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An Address

To the
ZOOLOGICAL SECTION OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

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Progressive Evolution and the Origin of Species.

The opening years of the present century have witnessed a remarkable development of Biology as an experimental science, a development which, however full of promise it may be for the future, for the time being appears to have resulted in a widespread disturbance of ideas which have themselves only recently succeeded in gaining general acceptance. The theory of organic evolution, plainly enough enunciated at the close of the eighteenth century and the beginning of the nineteenth century by Buffon, Lamarck and Erasmus Darwin, remained unconvincing to the great majority of thinking men until the genius of Charles Darwin not only brought together and presented the evidence in such a manner that it could no longer be ignored, but elaborated a logical explanation of the way in which organic evolution might be supposed to have taken place. Thanks to his labours and those of Alfred Russel Wallace, supported by the powerful influence of such men as Huxley and Hooker, the theory was placed upon a firm foundation, in a position which can never again be assailed with any prospect of success.

This statement is, I believe, entirely justified with regard to the theory of organic evolution itself, but the case is very different when we come to investigate the position of the various subsidiary theories which have been put forward from time to time with regard to what may perhaps be termed the modus operandi, the means by which organic evolution has been effected. It is in this field that controversy rages more keenly than ever before. Lamarck told us that evolution was due to the accumulated results of individual effort in response to a changing environment, and also to the direct action of the environment upon the organism. Darwin and Wallace taught us that species originated by the natural selection of favourable variations, and under the influence of Weismann's doctrine of the non-inheritance of acquired characters the theory of natural selection is in danger of becoming crystallised into an inflexible dogma. In recent years De Vries has told us that species arise by sudden mutations, and not by slow successive changes, while one of the most extreme opponents of "Mendelism," Professor Lotsy, lately informed us that all species arise by crossing, and seriously suggested that the vertebrate type arose by the crossing of two invertebrates!

This curious and many-sided divergence of opinion amongst expert biologists is undoubtedly largely due to the introduction of experimental methods into biological science. Such methods have proved very fruitful in results which at first sight seem to be mutually contradictory, and each group of workers has built up its own theory mainly on the basis of observations in its own restricted field.

Professor Bateson has said in his recently published "Problems of Genetics": "When . . . we contemplate the problem of Evolution at large the hope at the present time of constructing even a mental picture of that process grows weak almost to the point of vanishing. We are left wondering that so lately men in general, whether scientific or lay, were so easily satisfied. Our satisfaction, as we now see, was chiefly founded on ignorance."*

In view of this striking pronouncement on the part of one who devoted his life with signal success to the experimental investigation of evolutionary problems, the remarks which I propose to lay before you for your consideration to-day may well appear rash and ill-advised. I cannot believe, however, that the position is really quite so black as it is painted. We must perforce admit that the divers theories with regard to the working of organic evolution cannot all be correct in all their details, but it may be that each contains its own elements of truth, and that if these elements can but be recognised and sorted out, they may perhaps be recombined in such a form as to afford at any rate a plausible working hypothesis. We must bear in mind from the outset that in dealing with such a complex problem many factors have to be taken into account, and that widely different views on the question may be merely one-sided and not necessarily mutually exclusive.

I take it that there are three principal facts, or groups of facts, that have to be accounted for by any theory of organic evolution:—

(1) The fact that, on the whole, evolution has taken place in a progressive manner along definite and divergent lines.

(2) The fact that individual animals and plants are more or less precisely adapted in their organisation and in their behaviour to the conditions under which they have to live.

(3) The fact that evolution has resulted in the existence on the earth to-day of a vast number of more or less well-defined groups of animals and plants which we call species.

The first of these facts appears to me to be the most fundamental, and at the same time the one to which least attention is usually paid. The great question after all, is, Why do organisms progress at all instead of remaining stationary from generation to generation? To answer this question it is not necessary to go back to the beginning and consider the case of the first terrestrial organisms, whatever they may have been, nor are we obliged to take as illustrations the lowest organisms known to us as

*"Problems of Genetics," p. 97.

existing at the present day. We may consider the problem at any stage of evolution, for at each stage progress is, or may be, still taking place. We may even begin by considering what is usually regarded as the highest stage of all, man himself; and indeed this seems the most natural thing to do, for we certainly know more about the conditions of progress in man than in any other organism. I refer, of course, at the moment, not to progress in bodily organisation, but to progress in the ordinary sense of the word, the progress, say, of a family which rises in the course of a few generations from a position of obscure poverty to one of wealth and influence. You may perhaps say that such a case has no bearing upon the problem of organic evolution in a state of nature, and that we ought to confine our attention to the evolution of bodily structure and function. If so, I must reply that you have no right to limit the meaning of the term evolution in this manner; the contrast between man and nature is purely arbitrary; man is himself a living organism, and all the improvements that he effects in his own condition are part of the progress of evolution in his particular case. At any rate I must ask you to accept this case as our first illustration of a principle that may be applied to organisms in general.

If we inquire into the cause of the progress of our human family I think there can be only one answer —it is due to the accumulation of capital, or, as I should prefer to put it, to the accumulation of potential energy, either in the form of material wealth or of education. What one generation saves is available for the next, and thus each succeeding generation gets a better start in life, and is able to rise a little higher than the preceding one.

Every biologist knows, of course, that there are many analogous cases amongst the lower animals, and also amongst plants. The accumulation of food-yolk in the egg has undoubtedly been one of the chief factors in the progressive evolution of animals, although it has been replaced in the highest forms by a more effective method of supplying potential energy to the developing offspring. It may indeed be laid down as a general law that each generation, whether of animals or of plants, accumulates more energy than it requires for its own maintenance, and uses the surplus to give the next generation a start in life. There is every reason to believe that this has been a progressive process throughout the whole course of evolution, for the higher the degree of organisation the more perfect do we find the arrangements for securing the welfare of the offspring.

We cannot, of course, trace this process back to its commencement, because we know nothing of the nature of the earliest living things, but we may pause for a moment to inquire whether any phenomena occur amongst simple unicellular organisms that throw any light upon the subject. What we want to know is—How did the habit of accumulating surplus energy and handing it on to the next generation first arise?

Students of Professor H. S. Jennings' admirable work on the "Behaviour of the Lower Organisms" will remember that his experiments have led him to the conclusion that certain Protozoa, such as Stentor, are able to learn by experience how to make prompt and effective responses to certain stimuli; that after they have been stimulated in the same way a number of times they make the appropriate response at once without having to go through the whole process of trial and error by which it was first attained. In other words, they are able by pratice to perform a given action with less expenditure of energy. Some modification of the protoplasm must take place which renders the performance of an act the easier the oftener it has been repeated. The same is of course true in the case of the higher animals, and we express the fact most simply by saying that the animal establishes habits. From the mechanistic point of view we might say that the use of the machine renders it more perfect and better adapted for its purpose. In the present state of our knowledge I think we cannot go beyond this, but must content ourselves with recognising the power of profiting by experience as a fundamental property of living protoplasm.

It appears to me that this power of profiting by experience lies at the root of our problem, and that in it we find a chief cause of progressive evolution. Jennings speaks of the principle involved here as the 'Law of the readier resolution of physiological states after repetition," and, similarly, I think we must recognise a "Law of the accumulation of surplus energy" as resulting therefrom. Let us look at the case of the accumulation of food-yolk by the egg-cell a little more closely from this point of view. Every cell takes in a certain amount of potential energy in the form of food for its own use. If it leads an active life, either as an independent organism or as a constituent part of an organism, it may expend by far the greater part, possibly even the whole, of that energy upon its own requirements, but usually something is left over to be handed down to its immediate descendants. If, on the other hand, the cell exhibits very little activity and expends very little energy, while placed in an environment in which food is abundant, it will tend to accumulate surplus energy in excess of its own needs. Such is the case with the egg-cells of the multicellular animals and plants. Moreover, the oftener the process of absorbing food-material is repeated the easier does it become; in fact, the egg-cell establishes a habit of storing up reserve material or food-yolk. Inasmuch as it is a blastogenic character, there can be no objection to supposing that this habit will be inherited by future generations of egg-cells. Indeed we are obliged to assume that this will be the case, for we know that the protoplasm of each succeeding generation of egg-cells is directly continuous with that of the preceding generation. We thus get at any rate a possibility of the progressive accumulation of potential energy in the germ-cells of successive generations of multicellular organisms, and of course the same argument holds good with regard to successive generations of Protista.

It would seem that progressive evolution must follow as a necessary result of the law of the accu-

mulation of surplus energy in all cases where there is nothing to counteract that law, for each generation gets a better start than its predecessor, and is able to carry on a little further its struggle for existence with the environment. It may be said that this argument proves too much, that if it were correct all organisms would by this time have attained to a high degree of organisation, and that at any rate we should not expect to find such simple organisms as bacteria and amœbæ still surviving. This objection, which, of course, applies equally to other theories of organic evolution, falls to the ground when we consider that there must be many factors of which we know nothing which may prevent the establishment of progressive habits and render impossible the accumulation of surplus energy. Many of the lower organisms, like many human beings, appear to have an inherent incapacity for progress, though it may be quite impossible for us to say to what that incapacity is due.

It will be observed that in the foregoing remarks I have concentrated attention upon the storing up of reserve material by the egg-cells, and in so doing have avoided the troublesome question of the inheritance of so-called acquired characters. I do not wish it to be supposed, however, that I regard this as the only direction in which the law of the accumulation of surplus energy can manifest itself, for I believe that the accumulation of surplus energy by the body may be quite as important as a factor in progressive evolution as the corresponding process in the germ-cells themselves. The parents, in the case of the higher animals, may supply surplus energy, in the form of nutriment or otherwise, to the offspring at all stages of its development, and the more capital the young animal receives the better will be its chances in life, and the better those of its own offspring.

In all these processes, no doubt, natural selection plays an important part, but, in dealing with the accumulation of food material by the egg-cells, one of my objects has been to show that progressive evolution would take place even if there were no such thing as natural selection, that the slow successive variations in this case are not chance variations, but due to a fundamental property of living protoplasm and necessarily cumulative.

Moreover, the accumulation of surplus energy in the form of food-yolk is only one of many habits which the protoplasm of the germ-cells may acquire in a cumulative manner. It may learn by practice to respond with increased promptitude and precision to other stimuli besides that of the presence of nutrient material in its environment. It may learn to secrete a protective membrane, to respond in a particular manner to the presence of a germ-cell of the opposite sex, and to divide in a particular manner after fertilisation has taken place.

Having thus endeavoured to account for the fact that progressive evolution actually occurs by attributing it primarily to the power possessed by living protoplasm of learning by experience and thus establishing habits by which it is able to respond more quickly to environmental stimuli, we have next to inquire what it is that determines the definite lines along which progress manifests itself.

Let us select one of these lines and investigate it as fully as the time at our disposal will permit, with a view to seeing whether it is possible to formulate a reasonable hypothesis as to how evolution may have taken place. Let us take the line which we believe has led up to the evolution of air-breathing vertebrates. The only direct evidence at our disposal in such a case is, of course, the evidence of palæontology, but I am going to ask you to allow me to set this evidence, which, as you know, is of an extremely fragmentary character, aside, and base my remarks upon the ontogenetic evidence, which, although indirect, will, I think, be found sufficient for our purpose. One reason for concentrating our attention upon this aspect of the problem is that I wish to show that the recapitulation of phylogenetic history in individual development is a logical necessity if evolution has really taken place.

We may legitimately take the nucleated Protozoon cell as our starting-point, for, whatever may have been the course of evolution that led up to the cell, there can be no question that all the higher organisms actually start life in this condition.

We suppose, then, that our ancestral Protozoon acquired the habit of taking in food material in excess of its own requirements, and of dividing into two parts whenever it reached a certain maximum size. Here again we must, for the sake of simplicity, ignore the facts that even a Protozoon is by no means a simple organism, and that its division, usually at any rate, is a very complicated process. Each of the daughter-cells presently separates from its sister-cell and goes its own way as a complete individual, still a Protozoon. It seems not improbable that the separation may be due to the renewed stimulus of hunger, impelling each cell to wander actively in search of food. In some cases, however, the daughter-cells remain together and form a colony, and probably this habit has been rendered possible by a sufficient accumulation of surplus energy in the form of food-yolk on the part of the parent rendering it unnecessary for the daughter-cells to separate in search of food at such an early date. One of the forms of colony met with amongst existing Protozoa is the hollow sphere, as we see it, for example, in Sphærozoum and Volvox, and it is highly probable that the assumption of this form is due largely, if not entirely, to what are commonly called mechanical causes, though we are not in a position to say exactly what these causes may be. The widespread occurrence of the blastosphere or blastula stage in ontogeny is a sufficiently clear indication that the hollow, spherical Protozoon colony formed a stage in the evolution of the higher animals.

By the time our ancestral organism has reached this stage, and possibly even before, a new complication has arisen. The cells of which the colony is composed no longer remain all alike, but become differentiated, primarily into two groups, which we distinguish as somatic cells and germ-cells respectively.

From this point onwards evolution ceases to be a really continuous process, but is broken up into a series of ontogenies, at the close of each of which the organism has to go back and make a fresh start in the unicellular condition, for the somatic cells sooner or later become exhausted in their conflict with the environment and perish, leaving the germ-cells behind to take up the running. That the germ-cells do not share the fate of the somatic cells must be attributed to the fact that they take no part in the struggle for existence to which the body is exposed. They simply multiply and absorb nutriment under the protection of the body, and therefore retain their potential energy unimpaired. They are in actual fact, as is so often said, equivalent to so many Protozoa, and, like the Protozoa, are endowed with a potential immortality.

We know that, if placed under suitable conditions, or, in other words, if exposed to the proper environmental stimuli, these germ-cells will give rise to new organisms, like that in the body of which they were formerly enclosed. One of the necessary conditions is, with rare exceptions, the union of the germ-cells in pairs to form zygotes or fertilised ova; but I propose, in the first instance, for the sake of simplicity, to leave out of account the existence of the sexual process and the results that follow therefrom, postponing the consideration of these to a later stage of our inquiry. I wish, moreover, to make it quite clear that organic evolution must have taken place if no such event as amphimixis had ever occurred.

What, then, may the germ-cells be expected to do? How are they going to begin their development? In endeavouring to answer this question we must remember that the behaviour of an organism at any moment depends upon two sets of factors—the nature of its own constitution on the one hand, and the nature of its environment on the other. If these factors are identical for any two individual organisms, then the behaviour of these two individuals must be the same. If the germ-cells of any generation are identical with those of the preceding generation, and if they develop under identical conditions, then the soma of the one generation must also be identical with that of the other.† Inasmuch as they are parts of the same continuous germplasm—leaving out of account the complications introduced by amphimixis—we may assume that the germ-cells of the two generations are indeed identical in nearly every respect; but there will be a slight difference, due to the fact that those of the later generation will have inherited a rather larger supply of initial energy and a slightly greater facility for responding to stimuli of various kinds, for the gradual accumulation of these properties will have gone a stage further. The environment also will be very nearly identical in the two cases, for we know from experiment that if it were not the organism could not develop at all.

Throughout the whole course of its ontogeny the organism must repeat with approximate accuracy the stages passed through by its ancestors, because at every stage there will be an almost identical organism exposed to almost identical stimuli. We may, however, expect an acceleration of development and a slight additional progress at the end of ontogeny as the result of the operation of the law of the accumulation of surplus energy and of the slightly increased facility in responding to stimuli. The additional progress, of course, will probably be so slight that from one generation to the next we should be quite unable to detect it, and doubtless there will be frequent backslidings due to various causes.

We can thus formulate a perfectly reasonable explanation of how it is that the egg first undergoes segmentation and then gives rise to a blastula resembling a hollow protozoon colony; it does so simply because at every stage it must do what its ancestors did under like conditions. We can also see that progressive evolution must follow from the gradual accumulation of additions at the end of each ontogeny, these additions being rendered possible by the better start which each individual gets at the commencement of its career.

Let us now glance for a moment at the next stage in phylogeny, the conversion of the hollow spherical protozoon colony into the cœlenterate type of organisation, represented in ontogeny by the process of gastrulation. Here again it is probable that this process is explicable to a large extent upon mechanical principles. According to Rhumbler,* the migration of endoderm cells into the interior of the blastula is partly due to chemotaxis and partly to changes of surface tension, which decreases on the inner side of the vegetative cells owing to chemical changes set up in the blastocœl fluid.

We may, at this point, profitably ask the question, Is the endoderm thus formed an inherited feature of the organism? The material of which it is composed is of course derived from the egg-cell continuously by repeated cell-division, but the way in which that material is used by the organism depends upon the environment, and we know from experiment that modifications of the environment actually do produce corresponding modification in the arrangement of the material. We know, for example, that the addition of salts of lithium to the water in which certain embryos are developing causes the endoderm to be protruded instead of invaginated, so that we get a kind of inside-out gastrula, the well-known lithium larva.

It appears, then, that an organism really inherits from its parents two things: (1) a certain amount of protoplasm loaded with potential energy, with which to begin operations, and (2) an appropriate environment. Obviously, the one is useless without the other. An egg cannot develop unless it is provided with the proper environment at every stage. Therefore, when we say that an organism inherits a particular character from its parents, all we mean is that it inherits the power to produce that character under the influence of certain environmental stimuli.† The inheritance of the en-

†This is, of course, a familiar idea. Compare Driesch, Gifford Lectures, 1907, p. 214.

*Quoted by Przibram, "Experimental Zoology," English Trans., Part I., p. 47.

†Compare Dr. Archdall Reid's suggestive essay on "Biological Terms" ("Bedrock," January, 1914).

vironment is of at least as much importance as the inheritance of the material of which the organism is composed. The latter indeed is only inherited to a very small extent, for the amount of material in the egg-cell may be almost infinitesimal in comparison with the amount present in the adult, nearly the whole of which is captured from the environment and assimilated during ontogeny.

From this point of view the distinction between somatogenic and blastogenic characters really disappears, for all the characters of the adult organism are acquired afresh in each generation as a result of response to environmental stimuli during development. This is clearly indicated by the fact that you cannot change the stimuli without changing the result.

Time forbids us to discuss the phylogenetic stages through which the cœlenterate passed into the cœlomate type, the cœlomate into the chordate, and the chordate into the primitive vertebrate. We must admit that as yet we know nothing of the particular causes that determined the actual course of evolution at each successive stage. What we do know, however, about the influence of the environment, both upon the developing embryo and upon the adult, is sufficient to justify us in believing that every successive modification must have been due to a response on the part of the organism to some environmental change. Even if the external conditions remained practically identical throughout long periods of time, we must remember that the internal conditions would be different in each generation, because each generation starts with a slightly increase capital and carries on its development a little further under internal conditions modified accordingly.

At this point it may be asked, Is the response to environmental stimuli a purely mechanical one, and, if so, how can we account for the fact that at every stage in its evolution the organism is adapted to its environment? We shall have to return to this question later on, but it may be useful to point out once more that there is good reason to believe—especially from the experimental work of Jennings—that the response of even a unicellular organism to stimuli is to a large extent purposive; that the organism learns by experience, by a kind of process of trial and error, how to make the response most favourable to itself under any given change of conditions; in other words, that the organism selects those modes of response that are most conducive to its own well-being. Under the term response to stimuli we must, of course, include those responses of the living protoplasm which result in modifications of bodily structure, and hence the evolution of bodily structure will, on the whole, be of an adaptive character and will follow definite lines. There is good reason for believing, however, that many minor modifications in structure may arise and persist, incidentally as it were, that have no significance as adaptations.

One of the most remarkable and distinctive features of the lower vertebrates is the presence of gill-slits as accessory organs of respiration. These gill-slits are clearly an adaptation to aquatic life. When the ancestors of the higher vertebrates left the water and took to life on land the gills disappeared and were replaced by lungs, adapted for air-breathing. The change must, of course, have been an extremely gradual one, and we get a very clear indication of how it took place in the surviving dipnoids, which have remained in this respect in an intermediate condition between the fishes and the amphibia, possessing and using both gills and lungs.

We also know that even the most highly specialised air-breathing vertebrates, which never live in water and never require gills or gill-slits at all, nevertheless possess very distinct gill-slits during a certain period of their development. This is one of the most familiar illustrations of the law of recapitulation, and my only excuse for bringing it forward now is that I wish, before going further, to consider a difficulty—perhaps more apparent than real—that arises in connection with such cases.

It might be argued that if gill-slits arose in response to the stimuli of aquatic life, and if these stimuli are no longer operative in the case of air-breathing vertebrates, then gill-slits ought not to be developed at any stage of their existence. This argument is, I think, fully met by the following considerations.

At any given moment of ontogenetic development the condition of any organ is merely the last term of a series of morphogenetic stages, while its environment at the same moment—which of course includes its relation to all the other organs of the body—is likewise merely the last term of a series of environmental stages. We have thus two parallel series of events to take into consideration in endeavouring to account for the condition of any part of an organism—or of the organism as a whole—at any period of its existence:—

$E_1\ E_2\ E_3\ .\ .\ .\ .\ .\ .\ E_n$ environmental stages.
$M_1\ M_2\ M_3\ .\ .\ .\ .\ .\ M_n$ morphogenetic stages.

Ontogeny is absolutely conditioned by the proper correlation of the stages of these two series at every point, and hence it is that any sudden change of environment is usually attended by disastrous consequences. Thus, after the fish-like ancestors of air-breathing vertebrates had left the water and become amphibians, they doubtless still had to go back to the water to lay their eggs, in order that the eggs might have proper conditions for their development.

Obviously the environment can only be altered with extreme slowness, and one of the first duties of the parent is to provide for the developing offspring conditions as nearly as possible identical with those under which its own development took place. It is, however, inevitable that, as phylogenetic evolution progresses, the conditions under which the young organism develops should change. In the first place, the mere tendency to acceleration of development, to which we have already referred, must tend to dislocate the correlation between the ontogenetic series and the environmental series. Something of this kind seems to have taken place in the life-cycle of many Hydrozoa, resulting in the suppression of the free medusoid generation and the gradual degeneration of the gonophore. But it is probably in

most cases change in the environment of the adult that is responsible for such dislocation.

To return to the case of the amphibians. At the present day some amphibians, such as the newts and frogs, still lay their eggs in water, while the closely related salamanders retain them in the oviducts until they have developed into highly organised aquatic larvæ, or even into what is practically the adult condition. Kammerer has shown that the period at which the young are born can be varied by changing the environment of the parent. In the absence of water the normally aquatic larvæ of the spotted salamander may be retained in the oviduct until they have lost their gills, and they are then born in the fully-developed condition, while, conversely, the alpine salamander, whose young are normally born in the fully-developed state, without gills, may be made to deposit them prematurely in water in the larval, gill-bearing condition.

There can be no doubt that the ancestral amphibians laid their eggs in water in a completely undeveloped condition. The habit of retaining them in the body during their development must have arisen very gradually in the phylogenetic history of the salamanders, the period for which the young were retained growing gradually longer and longer. It is obvious that this change of habit involves a corresponding change in the environmental conditions under which the young develop, and in cases in which the young are not born until they have reached practically the adult condition this change directly affects practically the whole ontogeny. We may say that the series

$E_1\ E_2\ E_3\ .\ .\ .\ .\ .\ .\ .\ .\ E_n$ has become
$E_1'\ E_2'\ E_3'\ .\ .\ .\ .\ .\ .\ .\ .\ E_n'$.

and as the change of environment must produce its effect upon the developing organism the series

$M_1\ M_2\ M_3\ .\ .\ .\ .\ .\ M_n$ will have become
$M_1'\ M_2'\ M_3'\ .\ .\ .\ .\ .\ M_n'$.

We must remember that throughout the whole course of phylogenetic evolution this series is constantly lengthening, so that what was the adult condition at one time becomes an embryonic stage in future generations, and that the series thus represents not only the ontogeny, but also, though in a more or less imperfect manner, the phylogeny of the organism.

The character of each stage in ontogeny must depend upon (1) the morphological and physiological constitution of the preceding stage, and (2) the nature of the environment in which development is taking place. We cannot, however, distinguish sharply between those two sets of factors, for, in a certain sense, the environment gradually becomes incorporated in the organism itself as development proceeds, each part contributing to the environment of all the remainder, and the influence of this internal portion of the environment ever becoming more and more important.

The whole process of evolution depends upon changes of environment taking place so gradually that the necessary self-adjustment of the organism at every stage is possible. In the case of our amphibia the eggs could probably undergo the first stages of development, the preliminary segmentation, within the oviduct of the parent just as well as in the water, for in both cases they would be enclosed in their envelopes, and the morphological differences between the early stages in the two cases might be expected to be quite insignificant. But it must be the same at each term of the series, for each term is built upon the foundation of the preceding one, and the whole process takes place by slow and imperceptible degrees.

It is true that by the time we reach the formation of the vestigial gill-slits in the embryo of one of the higher vertebrates the environmental conditions are very different from those under which gill-slits were developed in their aquatic ancestors. But what then? Are not the gill-slits also very different? The changed environment has had its effect. The gills themselves are never developed, and the gill-slits never become functional; moreover, they disappear completely at later stages of development, when the conditions of life become still more different and their presence would be actually detrimental to their possessor. The embryo with the vestigial gill-slits is, as a whole, perfectly well adapted to its environment, though the gill-slits themselves have ceased to be adaptive characters. They still appear because the environmental conditions, and especially the internal conditions, which have now become far more important than the external ones, are still such as to cause them to do so.

I think the chief difficulty in forming a mental picture of the manner in which evolution has taken place, and especially in accounting for the phenomenon of recapitulation in ontogeny, which is merely another aspect of the same problem, arises from attempting to take in too much at once. There is no difficulty in understanding how any particular stage is related to the corresponding stage in the previous generation, and the whole series of stages, whether looked at from the ontogenetic or from the phylogenetic point of view, can be nothing else but the sum of its successive terms.

It will be convenient, before going further, to sum up the results at which we have so far arrived from the point of view of the theory of heredity. We have as yet seen no reason to distinguish between somatogenic and blastogenic characters. All the characters of the adult animal are acquired during ontogeny as the result of the reaction of the organism to environmental stimuli, both internal and external. All that the organism actually inherits is a certain amount of protoplasm—endowed with a certain amount of energy—and a certain sequence of environmental conditions. In so far as these are identical in any two successive generations the final result must be identical also, the child must resemble the parent; in so far as they are different the child will differ from the parent, but the differences in environment cannot be very great without preventing development altogether.

So far, it is clear, there has been no need to think of the germ-cells as the bearers of material factors or determinants that are responsible for the appearance of particular characters in the adult organism;

nor yet to suppose that they are, to use the phraseology of the mnemic theory of heredity, charged with the memories of past generations. They have been regarded as simple protoplasmic units, and the entire ontogeny has appeared as the necessary result of the reaction between the organism and its environment at each successive stage of development. This cannot, however, be a complete explanation of ontogeny, for if it were we should expect all eggs, when allowed to develop under the same conditions from start to finish, to give rise to the same adult form, and this we know is not the case. We know also, from observation and experiment, that the egg is in reality by no means a simple thing but an extremely complex one, and that different parts of the egg may be definitely correlated with corresponding parts of the adult body. It has been demonstrated in certain cases that the egg contains special organ-forming substances definitely located in the cytoplasm, and that if these are removed definite parts of the organism into which the egg develops will be missing. We know, also, that the nucleus of the germ-cell of either sex contains—at any rate at certain periods—a number of perfectly well-defined bodies, the chromosomes, and these also have been definitely correlated in certain cases with special features of the adult organisation.

Before we can hope to complete our mental picture of the manner in which organic evolution has taken place, if only in outline, it is evident that we must be able to account for the great complexity of structure which the germ-cells themselves have managed to acquire, and also to form some idea of the effect of this complication upon the development of both the individual and the race.

We must consider the origin of cytoplasmic and nuclear complications of the egg separately, for they appear to be due fundamentally to two totally distinct sets of factors. In the first place we have to remember that during oogenesis the egg-cell grows to a relatively large size by absorbing nutrient material from the body in which it is enclosed. It is this nutrient material that is used for building up the deutoplasm or food-yolk. There is good reason for believing that the character of this nutrient material will change, during the course of evolution, pari passu with the changing character of the organism by which it is supplied. Doubtless the change is of a chemical nature, for we know from precipitin experiments that the body fluids of closely allied species, or even of two sexes of the same species, de exhibit distinctly recognisable differences in chemical composition. It also appears highly probable, if not certain, from such experiments as those of Agar upon Simocephalus, that substances taken in with the food and which bring about conspicuous modifications of bodily structure, may at the same time be absorbed and stored up by the egg-cells so as to bring about corresponding changes in the adults into which the eggs develop.

There seems therefore to be no great difficulty in comprehending, at any rate in a general way, how the egg may become the repository of definite chemical substances, organ-forming substances if we like to call them so, possibly to be classed with the hormones and enzymes, which will influence the development in a particular manner as soon as the appropriate conditions arise.

Unfortunately, time will not allow of our following up this line of thought on the present occasion, but we may notice, before passing on, that with the accumulation of organ-forming substances in the egg we have introduced the possibility of changes in bodily structure, to whatever cause they may be due, being represented by correlated modifications in the germ-cells, and that is doubtless one of the reasons why the germ-cells of different animals are not all alike with regard to their potentialities of development.*.

We now come to the question of how the nucleus of the germ-cell acquired its great complexity of structure. We are not concerned here with the origin of the differentiation into nucleus and cytoplasm and the respective parts played by the two in the life of the cell. The problem which we have to consider is the complication introduced by the sexual process, by the periodically recurring union of the germ-cells in pairs, or, as Weismann has termed it, amphimixis. This is well-known to be essentially a nuclear phenomenon, in which the so-called chromatin substance is especially concerned, and it is a phenomenon which must have made its appearance at a very early stage of evolution, for it is exhibited in essentially the same manner alike in the higher plants and animals and in unicellular organisms.

Let us suppose, for the sake of argument, that when amphimixis first took place the chromatin of each germ-cell was homogeneous, but that it differed slightly in different germ-cells of the same species as a result of exposure to slightly different conditions during its past history. What would be likely to happen when two different samples of chromatin came together in the zygote? The result would surely depend upon the interaction of the complex colloidal multimolecules of which the chromatin is composed. Various possibilities would arise. (1) The two samples might differ in such a way as to act as poisons to one another, disturbing each other's molecular equilibrium to such an extent that neither could survive. This is possibly what happens when an ovum is fertilised by a spermatozoon of a distinct species, though there are, of course, exceptions. (2) They might be so alike as to be able to amalgamate more or less completely, so that there would simply be an increase of chromatin of possibly more or less modified constitution. (3) They might continue to exist side by side, each maintaining its own individual character.

In the third case the union of the two different samples would give rise to a mass of chromatin of twofold nature, and repetition of the process from generation to generation would, as Weismann has shown, result in ever-increasing heterogeneity, until the chromatin came to consist of a great number of different concrete particles, each of which might conceivably differ from all the others. But when

*Compare Cunningham's "Hormone Theory" of Heredity ("Archiv fuer Entwicklungsmechanik der Organismen," Bd. xxvi, Heft 3).

two heterogeneous masses of chromatin meet in the zygote there may be all sorts of mutual attractions and repulsions between the different colloidal multimolecules, for all three of our supposed cases may arise simultaneously, and thus the results may become extremely complicated.

The chromatin of the germ-cells in all existing organisms is undoubtedly heterogeneous, and this heterogeneity may be to some extent visibly expressed in its arrangements in more or less multiform chromosomes during mitosis. We may provisionally accept Weismann's view that these chromosomes are themselves heterogeneous, being composed of chromomeres or ids, which in their turn are composed of determinants.

All this complexity of structure may be attributed to the effects of oft-repeated amphimixis, a view which is supported in the most striking manner by the fact that the nucleus in all ordinary somatic cells (in animals and in the diploid generation of plants) has a double set of chromosomes, one derived from the male and the other from the female parent, and by the well-known phenomenon of chromatin reduction which always precedes amphimixis.

When we approach the problem of heredity from the experimental side we get very strong evidence of the existence in the germ-plasm of definite material substances associated with the inheritance of special characters. Mendelian workers generally speak of these substances as factors, but the conception of factors is evidently closely akin to that of Weismann's hypothetical determinants. The cytological evidence fits in very well with the view that the factors in question may be definite material particles, and it is quite possible that such particles may have a specific chemical constitution to which their effects upon the developing organism are due.

From our point of view the interesting thing is the possibility that arises through the sexual process of the permutation and combination of different factors derived from different lines of descent. A germ-cell may receive additions to its collection of factors or be subject to substractions therefrom, and in either case the resulting organism may be more or less conspicuously modified.

By applying the method of experimental hybridisation a most fruitful and apparently inexhaustible field of research has been opened up in this direction, in the development of which no one has taken a more active part than the present President of the British Association. There cannot be the slightest doubt that a vast number of characters are inherited in what is called the Mendelian manner, and, as they are capable of being separately inherited and interchanged with others by hybridisation, we are justified in believing that they are separately represented in the germ-cells by special factors. Important as this result is, I believe that at the present time there exists a distinct danger of exaggerating its significance. The fact that many new and apparently permanent combinations of characters may arise through hybridisation, and that the organisms thus produced have all the attributes of what we call distinct species, does not justify us in accepting the grotesque view—as it appears to me—that all species have arisen by crossing, or even the view that the organism is entirely built up of separately transmissible 'unit characters.'

Bateson tells up that 'Baur has for example crossed species so unlike as "Antirrhinum majus" and "molle," forms differing from each other in almost every feature of organisation.' Surely the latter part of this statement cannot be correct, for after all "Antirrhinum majus" and "molle" are both snapdragons, and exhibit all the essential characters of snapdragons.

I think it is a most significant fact that the only characters which appear to be inherited in Mendelian fashion are comparatively trivial features of the organisms which must have arisen during the last stages of phylogeny. This is necessarily the case, for any two organisms sufficiently nearly related to be capable of crossing are identical as regards the vast majority of their characters. It is only those few points in which they differ that remain to be experimented on. Moreover, the characters in question appear to be all non-adaptive, having no obvious relation to the environment and no particular value in the struggle for existence. They are clearly what Weismann calls blastogenic characters, originating in the germ-plasm, and are probably identical with the mutations of de Vries. These latter are apparently chromatin-determined characters, for as Dr. Gates has recently shown in the case of "Œnothera," mutation may result from abnormal distribution of the chromosomes in the reduction division.*

We have next to inquire whether or not the Mendelian results are really in any way inconsistent with the general theory of evolution outlined in the earlier part of this address. Here we are obviously face to face with the old dispute between epigenesis and preformation. The theory of ontogeny which I first put forward is clearly epigenetic in character, while the theory of unit characters, represented in the germ-cells by separate 'factors,' is hardly less clearly a theory of preformation, and of course the conception of definite organ-forming substances in the cytoplasm falls under the same category. The point which I now wish to emphasise is that the ideas of epigenesis and preformation are not inconsistent with one another, and that, as a matter of fact, ontogenetic development is of a dual nature, an epigenesis modified by what is essentially preformation.

We have already dealt briefly with the question of organ-forming substances in the cytoplasm, and it must, I think, be clear that the existence of these is in no way incompatible with a fundamental epigenesis. We shall find directly that the same is true of Mendelian 'factors' or Weismannian 'determinants.'

We have seen that it is possible to conceive of even a complex organism as inheriting nothing from its parent but a minute speck of protoplasm, endowed with potential energy, and a sequence of suitable environments, the interaction between the two bringing about a similar result in each succeeding generation, with a slow progressive evolution due to the operation of the law of accumulation of surplus energy. If any of the conditions of develop-

*"Quarterly Journal of Microscopical Science," vol. 59, p. 537.

ment are changed the result, as manifested in the organisation of the adult, must undergo a corresponding modification. Suppose that the chromatin substance of the zygote is partially modified in molecular constitution, perhaps by the direct action of the environment, as appears to happen in the case of Tower's experiments on mutation in the potato beetle, or by the introduction of a different sample of chromatin from another individual by hybridisation. What is the germ-plasm now going to do? When and how may the changes that have taken place in its constitution be expected to manifest themselves in the developing organism?

Let us consider what would be likely to happen in the first stages of ontogeny. If the germ-plasm had remained unaltered the zygote would have divided into blastomeres under the stimuli of the same conditions, both internal and external, as those under which the corresponding divisions took place in preceding generations. Is the presence of a number of new colloidal multimolecules in the germ-plasm going to prevent this? The answer to this question probably depends partly upon the proportion that the new multimolecules bear to the whole mass, and partly upon the nature of the modification that has taken place. If the existence of the new multimolecules is incompatible with the proper functional activity of the germ-plasm as a whole there is an end of the matter. The organism does not develop. If it is not incompatible we must suppose that the zygote begins its development as before, but that sooner or later the modification of the germ-plasm will manifest itself in the developing organism, in the first instance as a mutation. In cases of hybridisation we may get a mixture in varying degrees of the distinguishing characters of the two parent forms, or we may get complete dominance of one form over the other in the hybrid generation, or we may even get some new form, the result depending on the mutual reactions of the different constituents of the germ-plasm.

The organism into which any zygote develops must be a composite body deriving its blastogenic characters from different sources; but this cannot affect its fundamental structure, for the two parents must have been alike in all essential respects or they could not have interbred, and any important differences in the germ-plasm must be confined to the 'factors' for the differentiating characters. The fundamental structure still develops epigenetically on the basis of an essentially similar germ-plasm and under essentially similar conditions as in the case of each of the two parents, and there is no reason to suppose that special 'factors' have anything to do with it.

We thus see how new unit characters may be added by mutation and inter-changed by hybridisation while the fundamental constitution of the organism remains the same and the epigenetic course of development is not seriously affected. All characters that arise in this way must be regarded, from the point of view of the organism, as chance characters due to chance modifications of the germ-plasm, and they appear to have comparatively little influence upon the course of evolution.

One of the most remarkable features of organic evolution is that it results in the adaptation of the organism to its environment, and for this adaptation mutation and hybridisation utterly fail to account. Of course the argument of natural selection is called in to get over this difficulty. Those organisms which happen to exhibit favourable mutations will survive and hand on their advantages to the next generation, and so on. It has frequently been pointed out that this is not sufficient. Mutations occur in all directions, and the chances of a favourable one arising are extremely remote. Something more is wanted, and this something, it appears to me, is to be found in the direct response of the organism to environmental stimuli at all stages of development, whereby individual adaptation is secured, and this individual adaptation must arise again and again in each succeeding generation. Moreover, the adaptation must, as I pointed out before, tend to be progressive, for each successive generation builds upon a foundation of accumulated experience and has a better start than its predecessors.

Of course natural selection plays its part, as it must in all cases, even in the inorganic world, and I believe that in many cases—as for example in protective resemblance and mimicry—that part has been an extremely important one. But much more important than natural selection appears to me what Baldwin* has termed 'Functional Selection,' selection by the organism itself, out of a number of possible reactions, of just those that are required to meet any emergency. As Baldwin puts it, 'It is the organism which secures from all its overproduced movements those which are adaptive and beneficial. Natural selection is here replaced by intelligent selection, for I think we must agree with Jennings† that we cannot make a distinction between the higher and the lower organisms in this respect, and that all purposive reactions, or adjustments, are essentially intelligent.

Surely that much-abused philosopher, Lamarck, was not far from the truth when he said, 'The production of a new organ in an animal body results from a new requirement which continues to make itself felt, and from a new movement which this requirement begets and maintains.'‡ Is not this merely another way of saying that the individual makes adaptive responses to environmental stimuli? Where so many people fall foul of Lamarck is with regard to his belief in the inheritance of acquired characters. But in speaking of acquired characters Lamarck did not refer to such modifications as mutilations; he was obviously talking of the gradual self-adjustment of the organism to its environment.

We are told, of course, that such adjustments will only be preserved so long as the environmental stimuli by which they were originally called for continue to exercise their influence. Those who raise this objection are apt to forget that this is exactly what happens in evolution, and that the sine quo non of development is the proper maintenance of the ap-

*"Development and Evolution" (New York, 1902), p. 87.
†"Behaviour of the Lower Organisms" (New York, 1906), pp. 334, 335.
‡"Histoire naturelle des Animaux sans Vertebres, tom. I. 1815, p. 185.

propriate environment, both internal and external. Natural selection sees to it that the proper conditions are maintained within very narrow limits.

A great deal of the confusion that has arisen with regard to the question of the inheritance of acquired characters is undoubtedly due to the quite unjustifiable limitation of the idea of 'inheritance' to which we have accustomed ourselves. The inheritance of the environment is, as I have already said, just as important as the inheritance of the material foundation of the body, and whether or not a newly acquired character will be inherited must depend, usually at any rate, upon whether or not the conditions under which it arose are inherited. It is the fashion nowadays to attach very little importance to somatogenic characters in discussing the problem of evolution. The whole fundamental structure of the body must, however, according to the epigenetic view, be due to the gradual accumulation of characters that arise as the result of the reactions of the organism to its environment, and which are therefore somatogenic, at any rate in the first instance, though there is reason to believe that some of them may find expression in the germ-cells in the formation of organ-forming substances, and possibly in other ways. Blastogenic characters which actually originate in the germ-cells appear to be of quite secondary importance.

We still have to consider the question, How is it that organic evolution has led to the formation of those more or less well-marked groups of organisms which we call species? We have to note in the first place that there is no unanimity of opinion amongst biologists as to what a species is. Lamarck insisted that nature recognises no such things as species, and a great many people at the present day are, I think, still of the same opinion. In practice, however, every naturalist knows that there are natural groups to which the vast majority of individuals can be assigned without any serious difficulty. Charles Darwin maintained that such groups arose, under the influence of natural selection, through gradual divergent evolution and the extinction of intermediate forms. To-day we are told by de Vries that species originate as mutations which propagate themselves without alteration for a longer or shorter period, and by Lotsy that species originate by crossing of more or less distinct forms, though this latter theory leaves quite unsolved the problem of where the original forms that crossed with one another came from.

I think a little reflection will convince us that the origin of species is a different problem from that of the cause of progressive evolution. We can hardly doubt, however, that Darwin was right in attributing prime importance to divergent evolution and the disappearance of connecting links. It is obvious that this process must give rise to more or less sharply separated groups of individuals to which the term species may be applied, and that the differences between these species must be attributed ultimately to differences in the response of the organism of differing conditions of the environment. It may be urged that inasmuch as different species are often found living side by side under identical conditions the differences between them cannot have arisen in this way, but we may be quite certain that if we knew enough of their past history we should find that their ancestors had not always lived under identical conditions.

The case of flightless birds on oceanic islands is particularly instructive in this connection. The only satisfactory way of explaining the existence of such birds is by supposing that their ancestors had well developed wings, by the aid of which they made their way to the islands from some continental area. The conditions of the new environment led to the gradual disuse and consequent degeneration of the wings until they either became useless for flight or, in the case of the moas, completely disappeared. It would be absurd to maintain that any of the existing flightless birds are specifically identical with the ancestral flying forms from which they are descended, and it would, it appears to me, be equally absurd to suppose that the flightless species arose by mutation or by crossing, the same result being produced over and over again on different islands and in different groups of birds. This is clearly a case where the environment has determined the direction of evolution.

In such cases there is not the slightest ground for believing that crossing has had anything whatever to do with the origin of the different groups to which the term species is applied; indeed the study of island faunas in general indicates very clearly that the prevention of crossing, by isolation, has been one of the chief factors in the divergence of lines of descent and the consequent multiplication of species, and Romanes clearly showed that even within the same geographical area an identical result may be produced by mutual sterility, which is the cause, rather than the result, of specific distinction.

Species, then, may clearly arise by divergent evolution under changing conditions of the environment, and may become separated from one another by the extinction of intermediate forms. The environmental stimuli (including, of course, the body as part of its own environment) may, however, act in two different ways: (1) Upon the body itself, at any stage of its development, tending to cause adaptation by individual selection of the most appropriate response; and (2) upon the germ-plasm, causing mutations or sudden changes, sports, in fact, which appear to have no direct relation whatever to the well-being of the organism in which they appear, but to be purely accidental. Such mutations are, of course, inherited, and, inasmuch as the great majority of specific characters appear to have no adaptive significance, it seems likely that mutation has had a great deal to do with the origin of species, though it may have had very little to do with progressive evolution.

Similarly with regard to hybridisation, we know that vast numbers of distinct forms, that breed true, may be produced in this way, but they are simply due to recombinations of mutational characters in the process of amphimixis, and have very little bearing upon the problem of evolution. If we like to call the new groups of individuals that originate

thus "species," well and good, but it only means that we give that name, as a matter of convenience, to any group of closely related individuals which are distinguished by recognisable characters from the individuals of all other groups, and which hand on those characters to their descendants so long as the conditions remain the same. This, perhaps, is what we should do, and just as we have learnt to regard individuals as the temporary offspring of a continuous stream of germ-plasm, so we must regard species as the somewhat more permanent but nevertheless temporary offshoots of a continuous line of progressive evolution. Individuals are to species what the germ-plasm is to individuals. One species does not arise from another species, but from certain individuals in that species, and when all the individuals become so specialised as to lose their power of adaptation, then changes in the environment may result in the extinction of that line of descent.

It is hardly necessary to point out that no explanation that we are able to give regarding the causes of either phylogenetic or ontogenetic evolution can be complete and exhaustive. Science can never hope to get to the bottom of things in any department of knowledge; there is always something remaining beyond our reach. If we are asked why an organism chooses the most appropriate response to any particular stimulus, we may suggest that this is the response that relieves it from further stimulation, but we cannot say how it learns to choose that response at once in preference to all others. If we are asked to account for some particular mutation, we may say that it is due to some modification in the constitution or distribution of the chromosomes in the germ-cells, but even if we knew exactly what that modification was, and could express it in chemical terms, we could not really say why it produces its particular result and no other, any more than the chemist can say why the combination of two gases that he calls oxygen and hydrogen gives rise to a liquid that he calls water.

There is one group of ontogenetic phenomena in particular that seem to defy all attempts at mechanistic interpretation. I refer to the phenomena of restitution, the power which an organism possesses of restoring the normal condition of the body after it has been violently disturbed by some external agent. The fact that a newt is able to regenerate its limbs over and over again after they have been removed, or that an echinoderm blastula may be cut in half and each half give rise to a perfect larva, is one of the most surprising things in the domain of biological science. We cannot, at present at any rate, give any satisfactory mechanistic explanation of these facts, and to attribute them to the action of some hypothetical Entelechy, after the manner of Professor Hans Driesch, is simply an admission of our inability to do so. We can only say that in the course of its evolution each organism acquires an individuality or wholeness of its own, and that one of the fundamental properties of living organisms is to maintain that individuality. They are able to do this in a variety of ways, and can sometimes even replace a lost organ out of material quite different from that form which the organ in question is normally developed, as in the case of the regeneration of the lens of the eye from the iris in the newt. That there must be some mechanism involved in such cases is, of course, self-evident, and we know that that mechanism may sometimes go wrong and produce monstrous and unworkable results; but it is, I think, equally evident that the organism must possess some power of directing the course of events, so as generally to secure the appropriate result; and it is just this power of directing chemical and physical processes, and thus employing them in its own interests, that distinguishes a living organism from an inanimate object.

In conclusion I ought, perhaps, to apologise for the somewhat dogmatic tone of my remarks. I must ask you to believe, however, that this does not arise from any desire on my part to dogmatise, but merely from the necessity of compressing what I wished to say into a totally inadequate space. Many years of patient work are still needed before we can hope to solve, even approximately, the problem of organic evolution, but it seemed to me permissible, on the present occasion, to indulge in a general survey of the situation, and see how far it might be possible to reconcile conflicting views and bring together a number of ideas derived from many sources in one consistent theory.

British Association for the Advancement of Science.

ZOOLOGY SECTION.

(By our Special Correspondent.)

Tuesday, August 18, 1914.

The discussion on the origin and nature of species, before the combined sections of Zoology and Botany, proved to be one of the most important of the Melbourne meeting. The president, Professor Bateson, had virtually flung down the gauntlet to the evolutionists, and the public interest aroused drew a crowded house. Nor were members disappointed, for at this full-dress debate, equivalent in a sense to a discussion of the first part of the Presidential Address, the level of the speeches was very high indeed, and the majority of the experts on heredity took part.

Dr. Rendle, F.R.S., led off with a careful review of the progress of our knowledge of heredity, with critical comments on the ideas on mutation and variation of de Vries and his school.

Jourdan, in his plant studies, had isolated 200 subspecies, but the features on which his distinctions into species depended were simply those of fluctuating variability. No new character had appeared in this species, though cultivated for ten years; it remained constant under these conditions.

Vettstein's studies of gentians and the like flowering in alpine meadows before and after mowing showed, he thought, merely a lateral process of selection of slight variations and were not a parallel, nor could this be accepted as evidence of the origin of species.

Lotse took stable species, and by crossing produced a series of individual plant hybrids. For example, in antirrhinum an indiscriminate series of new forms had resulted But these were really out of line with their environment; 90 per cent. were absolute monstrosities, and, further, no evidence could be gleaned even from hybrids now occurring in nature.

The idea of the production of species by a continued omission of factors showed a sublime indifference to the rules of mathematics.

Professor Dendy gave a very guarded statement of his position. As to the definition of species, a word was a sort of mythical monster; as for example in spirostella one investigator said 32 species were identical with certain "tropes." The only safe generalisation was that species had arisen by a process of evolution. Evolution, however, was probably of a dual nature; first the fundamental method of Darwin, epigenetic, the adaptation to constantly changing surroundings, and second the profound modification, introduced by sexual reproduction, with its permutations and combinations, superimposed on the fundamental evolution as shown in the Mendelian phenomena.

As an example of the former he gave the case of wingless birds in New Zealand, emphasised by the fact of the appearance of the same result in different islands.

In the second case there were the results of hybridization, mutation appearing by the influence of the germ-plasm. This was evidenced in Agar's work on Cynocephalus, and the influence of diet on the eggs.

Species could not be defined; it represented different things in different circumstances, and was merely a convenient term.

Professor Bateson took up the position that "we don't know." It was not a question of the actual definition of species, but rather whether we could define it. It was necessary to submit this to experimental analysis, and genetics was the best method. He thought Dr. Rendle's criticism of the specific characters of plants showed some confusion. In the case of antirrhinum, he had criticised the characters selected for distinction of species, and emphasised the value of the appearance of the seed, yet this very instance, crinkled or smooth, or green or yellow, was in the sweet pea the classical example of Mendelian discrimination. Bateson criticised the systematists severely. The name was to the plant what the pin was to the butterfly, a mere peg to attach our ideas to; most of it was pure pragmatism and a mere catalogue.

De Vries showed the differences to be, first genetic and second fluctuating. We might be able by genetic analysis to show which characters remained unshaken. We might not offer anything very satisfactory at present, but it was apparent that the origin of variation was not as we thought before, e.g., in the case of polymorphic moths. The only actual examples, so far, that could be given were definitely due to a process of loss. To the statement that the answer would be zero, he instanced the example of the apple and pear in the wild state; the systematist might easily classify these together, but genetically they were very different. What we perceived was no criterion.

Professor Poulton gave the example of the kaleidoscope, where each pattern influenced the succeeding one. Fluctuations occurred, and then came a total change of pattern, which was practically a variation. Much of this seemed to be like the unpacking of the original complex. A sort of new genetic level was thus reached. In the case of the original the change was more limited. Self-adaptation by environmental stimulus alone was doubtful; selection of an inherited variation must be superadded. Lotse's crossing hypothesis could hardly explain the extreme variability in island forms, as the species must at first have been few. The best idea of a species of animals was that of a syngamic community, i.e., a group in which inter-breeding took place.

Professor Benjamin Moore, who was asked to represent the physiological side, made a very interesting contribution. He compared his position to the President's remark regarding Harry Lauder's story of the boy's motor-car, which he represented by a washer. The chief mistake in dealing with species was that of relying on mere inspection. It was just as if a motor-car were judged merely by the colour of the paint, instead of by a consideration of its design and works. He mentioned his idea of the origin of life in the occurrence of protozoa in the sun's heat and simplest forms of life at the temperature of boiling water and the need of getting down to the basal outline of evolution. Biologists were in the same position as physicists and chemists were a century ago. First came the appearance of simple, inorganic compounds, and their colloids. He thought that two paths probably existed: one, the long path of evolution, as evidenced in geological observations, and the other the short path of genetic or hereditary causes.

Professor Minchin dealt briefly with an aspect not previously touched upon, namely, the most primitive living organisms, the protista, the two groups of which—bacteria and protozoa—showed the primary difference in reproduction; in protozoa by a definite sex method, while in bacteria no such sex differentiation occurred. In the lower grade organisms without sex, the possibility of variation seemed to be infinite, while true species occurred when sex was present, e.g., in the dog. Sex and the effect of crossing introduced a levelling down of variation up to a certain point, and led to a stability essential to the formation of a species.

Dr. Dixey spoke of the difficulties met with in applying ideas from animals to plants. While Linnæus had fixed the "species," it was found that transition forms had to be recognized. Evolution had greatly increased the difficulties of classification. Some species seemed to be quite circumscribed, while in the other the specific character appeared to be ill-defined and variable. In quoting entomological species, he instanced coleas, the cloudy yellow butterfly and the two palæoarctic types, with their infinite gradations. Many species were divided into groups, according to the different geographical localities where they occurred. For example, the swallow-tail butterfly in Socotra and on the mainland exhibited constant differences. Bateson's idea of fractionations still lacked experimental proof. While the selection of small but heritable variations seemed possible, Lamarchism could not be accepted at present.

Mr. Gregory, a Mendelian worker, criticized some of the previous species. He acknowledged not species as stated by systematism, but the existence of some entity, a faith gained by handling plants and animals. Breeding alone was not a safe test, as instances of self-sterile forms in which sterility was an inherited factor, were recognized. With regard to the adaptation idea of a long series of gradual forms, a long time was postulated. The process was stated to be going on so slowly that it is not seen. The speaker stated that this was equivalent to assuming that it could no tbe seen at all.

Dr. Cunningham supported the general evolutionary theory. The existence of permanent indivisible units which are not affected by external stimuli was assumed by Mendelians. They considered that the specific characters were due to these gametes. The permanent units, however, had not yet been established, while function and adaptation were ignored. For example, in crosses in fowl, the recessive white was not a pure white; a certain small and varying admixture of colour was often produced, and frequently an infinite series of gradations was met with. Again, the origin of positive characters in individuals of darker colour than the original was not fully explained. In evolution, the origin of additional characters was not a difficulty. He made a strong point of the failure of the Mendelian to explain the correspondence of mutations to external conditions, e.g., the appearance of lungs in the tadpole, where, owing to the conditions obtaining, a gill would have been expected. How else could this metamorphosis occur except it be conditioned by environment? Again, in the instance of secondary sexual characters, e.g., the antlers of the stag, no reason was given for the difference between male and female, for both inherited the factor. It seemed that some external conditions, some mechanical somatic irritation, must be responsible for the physiological development of the antler. Some evidence for Lamarchism seemed present in their symmetrical character. The tendency to go farther than this seemed to him to be unnecessary.

Dr. Bruce, as a British observer for very many years, gave his experience of the working of the idea of species. The Linnæan arrangement had proved grotesque. Many animals and plants known to be hybrids, for example, had Linnæan specific names. Because a variation was small, it was not necessarily ephemeral, for the same variations of certain seeds that were found in neolithic graves were met with at the present day.

Dr. Rendle, in reply, still upheld the view of the systematist. Species was not merely a label, as, e.g., the specific characters of orchids. De Vrees was confusing in that he alluded to fluctuations as inherited for a short time only. While genetics had brought a valuable laboratory point of view before the naturalist, he though it as neces-

(Continued on page 239.)

Medical Journal of Australia.

SATURDAY, SEPTEMBER 5, 1914.

The Enteric Fever Problem.

The dangers from epidemic diseases to which the armies of the world are at present exposed are even greater than those arising from the wounds and injuries inflicted by modern projectiles. Since the theatre of the war—as far as can be judged at present—is likely to be situated mainly in the northern portions of Europe, the chief diseases to which the troops will be exposed are enteric fever and dysentery. The responsibility of this fact lies at the door of the health authorities of the various nations. It is possible to stamp out typhoid fever, provided that the sanitary service is assisted by the medical profession and the public, and provided that sufficient money is placed at the disposal of those conducting the campaign. The disease can only be attacked by prophylactic measures, and while certain direct methods have succeeded in the past in reducing the incidence of the disease in many areas, it has become obvious that every prophylactic measure must be employed before complete success can be effected. Thanks to a rigid control of the water supply, carried out by Dr. Houston in London, and also to efficient notification, isolation and disinfection, added to an almost perfect system of sewage disposal, London has been nearly freed from the disease. But not quite. From time to time, cases occur and give rise to smaller or larger epidemics. The infection is at times traced to carriers; at other times, it is spread by means of the house fly or other vector; at times food, including milk, is the means of propagating the infection. The large cities of the Commonwealth, too, are kept practically free from any serious outbreak. The dangerous sources of infection are to be sought in rural districts, and often in asylums, where it is difficult to obtain the co-operation of the public or of the inmates to check the spread by the observation of simple but important hygienic rules. It must be remembered that as long as the civil community is attacked by enteric fever, so long will the disease break out among troops wherever war takes place. The duty of the sanitarian is therefore twofold. He has to stay the spread of infection, and stamp out epidemics locally, and he has to render the soldier on service as immune to the disease as possible. A very large literature on the subject of the etiology and epidemiology of enteric fever exists. Too large perhaps for practical purposes, for among the almost countless articles, pamphlets, and books dealing with this subject, an important fact may easily be missed. The Department of Public Health of New South Wales has just reproduced in its third annual report an excellent paper by Dr. Burton Bradley, in which much valuable information in connexion with the epidemiology is contained. In another column we publish a short resume of the conditions obtaining at present in various country districts of New South Wales. From these and other sources, the extent of the disease and the sources of infection can be accurately measured.

In order to recognize the problem in its full significance, it is necessary to recall that the disease is caused by the bacillus typhosus discovered by Eberth in 1880. In pure water, the bacillus has but a short life, while in sewage and soil it persists for a long time. On the other hand, the rays of the sun and the action of certain protozoa are potent germicidal agents. The bacilli leave the patient or the carrier through the excretions. For all practical purposes, enteric fever may be regarded as non-contagious. Vincent and Flexner first discovered the bacilli in the blood of patients (1893), and it has long been recognized that the urine of persons either suffering or convalescing from typhoid fever contain enormous numbers of bacilli. It is necessary, therefore, to render both the urine and fæces of those who harbour Eberth's bacilli harmless. When this is not done, the infecting agent may be carried directly to food or water, or indirectly through the agency of flies and other insects. In a district without an adequate system of sewerage, and especially where pans are substituted for earth privies, the difficulty of the prevention of fly infection becomes considerable. Flies can scarcely be exterminated. Pans must be accessible to flies, at all events during use, save when the closet is quite dark, and this is extremely difficult to secure uniformly among a rural and perhaps not very well-

to-do population. It is not easy to detect every case of mild, atypical enteric infection and every carrier; and even when the source of infection has been recognized, it may prove extremely difficult to enforce such measures as would effectively render the person harmless to those living in the neighbourhood, especially in sparsely populated districts. When the local authority, like that at Broken Hill (see page 240), neglects the obvious precautions recommended by the medical officer of health, the problem becomes almost hopeless. Ideally, there should be no practical difficulty in detecting every source of infection, in rendering this source harmless and in instituting such preventive measures as would safeguard the community against an outbreak. But in practice, this cannot be done in some of the rural districts.

It, therefore, becomes necessary to utilize another weapon to attain success. This is the prophylactic inoculation of killed b. typhosus. Almroth Wright, and, independently, Pfeiffer and Kolle, discovered that killed cultures of a 48 hours growth of b. typhosus injected in suitable doses lend a powerful protection against the disease, which is said to last for about 1 or 2 years. The dosage of this vaccine is described in some detail on page 238. Before such a procedure could be recommended for general application, it is necessary to enquire what degree of protection it can afford, and in how far it can be regarded as a harmless procedure. Wright has, fortunately for medicine of the present day, applied prophylactic vaccination against enteric fever on a large scale in the British Army, both in India and in South Africa. In 1899, the incidence of enteric fever among the unprotected soldiers of the Indian Army was 2.54 per cent., and the mortality 0.56 per cent. 4502 men were inoculated, and the incidence of the disease during the year among this number was reduced to 0.98 per cent., with a mortality of 0.2 per cent. A similar striking reduction in morbidity and mortality was effected in 1900, 1901 and subsequent years. During the Boer War, there were 12,234 troops in the garrison of Ladysmith. Of these, 10,529 were unprotected, and 14.14 per cent. were attacked with enteric fever. The mortality was 3.12 per cent. The remaining 1705 had been inoculated, and only 2.05 per cent. were attacked, and 0.47 per cent. died. In the various field and military hospitals in South Africa, the mortality among the inoculated soldiers was persistently lower than among the unprotected. Thus, at Natal, it was 2.18 per cent., as against 5.7 per cent.; at Bloemfontein it was 5.6 per cent., as compared with 14.0 per cent. In regarding the figures, however, it is necessary to recognize the fact that enteric fever only attacked two or three hundred soldiers who had been treated by preventive vaccination, and that a very small number of these men died. On the other hand, the unprotected soldiers were attacked wholesale, and approximately 8,000 died of the disease. In German West Africa, Kuhn inoculated 7,287 soldiers, and affected thereby a large reduction in the incidence and mortality of the disease. He was able to record that in over 50 per cent. of the cases affecting the inoculated men, the attacks were mild, while only 36.55 per cent. of the unprotected patients had mild attacks. It appears, therefore, that prophylactic inoculation reduces the frequency of infection considerably, and further renders an attack less severe than it would otherwise have been. In regard to the question of the safety of the inoculation, those who have observed the persons after injection agree that, with the exception of slight temporary disturbance of the general health, the procedure is quite safe and harmless.

The Department of Public Health of New South Wales has determined that, since the sanitary and other preventive measures which have led to a control of the infection in well ordered cities cannot be applied at present in many country districts, an attempt must be made to raise the immunity of the whole population in those districts where the disease is endemic. Vaccine has been prepared, and will be handed over free of charge to private practitioners, if they will use it. The Department is determined that full use shall be made of this method of prevention. The practioner will have no difficulty in getting his patients to submit to the inoculation, provided that he explains the position clearly and with emphasis, and provided that his fees are not too high. If after a reasonable time the majority of the population in the affected districts is not vaccinated against enteric fever, other steps will be adopted. The Department will carry

out the prophylactic treatment by its own officers free of charge, and the private practitioners will only have themselves to blame for not having seized the opportunity while it lasted.

Prophylactic inoculation is, thus, to be carried out among the civil population and among the troops in the field. This measure will effect a considerable reduction in the morbidity and mortality of the disease, but it will not stamp it out. The sincere hope must be expressed that war will not again occur between the Great Powers of the World. Precaution must, nevertheless, be taken to rob it of one of its horrors by the stamping out of enteric fever among the civil community. This cannot be effected alone by prophylactic inoculation, but every means must be employed before success can be relied on. One of the most powerful of the available means is strict cleanliness, and the local authorities, as well as the individual members of the community, must be compelled to practice this.

THE TASMANIAN HOSPITALS.

A Royal Commission has been appointed for the purpose of enquiring into the methods of administration adopted in the hospitals in the State of Tasmania. This enquiry is now being undertaken by the Commissioner — Dr. McClintoch, the Chief Health Officer of the State. Good work is undoubtedly being carried out in the various hospitals, but a number of administrative and other matters in connexion with the institutions require attention. In the two principal hospitals in Hobart and Launceston, the Boards of Directors are nominated and elected by their own members. It need not be pointed out that such a system is objectionable in every respect, and must be altered. We are informed that a few years ago their powers and functions were grossly misapplied, and that irregularities extended to the audit department. It is stated that the Government then in power were cognisant of these defects, but neglected to make adequate enquiries into the matter, and, consequently, no reforms were instituted. These and other matters will now be thoroughly investigated, and the Commissioner will, we trust, deal frankly and fearlessly with all the defects discovered. It is always a matter of great regret when hospitals are not properly managed, and, in the interests of the public and of the profession no pains should be spared to place them on a thoroughly satisfactory footing. In regard to the hospital in Hobart, various complaints have been made. Of these, one affects the professional side of the hospital, rather than the administrative. It is a matter which would be likely to arouse a false impression in the lay mind. A patient was treated for two months for peripheral neuritis, and at the end of this time was said to have been discharged as incurable. The patient was subsequently admitted into the Homeopathic Hospital, and while in this institution, recovery took place. We have no information as to the cause of the peripheral neuritis, nor have we reliable evidence as to whether the case was regarded as incurable when the patient was discharged from the Hobart Hospital. A cautious neurologist will always be extremely guarded in giving a bad prognosis in cases of neuritis, but even when a human mistake of this nature be made, it is unusual for a complaint to arise out of it. The public, however, must be impressed with the fact that recovery occurring in such cases is spontaneous, and is rarely, if ever, influenced by treatment. Charlatans and medical practitioners who follow unorthodox lines of treatment often attempt to make capital out of accidents of this type. The medical profession must counteract any claims of this kind by very definite pronouncements. The report of the Royal Commissioner on the hospital question will be of interest to the profession, but the action of the Government in regard to the recommendations will be of still greater importance.

THE HEALTH OF THE EXPEDITIONARY FORCE.

Pamphlets are about to be distributed to the Australian Expeditionary Force, with simple rules for the preservation of health on field service. These rules have been drawn up on sound lines, and are expressed in simple language. They deal with personal cleanliness, the hygiene of the camp, the measures to be adopted to prevent infection and to preserve activity and similar matter. The necessity of keeping food and water for the end of a march, of never drinking unboiled water, and of a cleanly and proper selection of the place to discharge the excreta are emphasised in a very proper manner. These pamphlets should serve a very useful purpose in preserving the health of our men on service, and while it cannot be expected that they will prevent the outbreak of infective disease, it is at least reasonable to anticipate that the rules will be observed to some extent, and that by these means disease will be lessened, and the physique of the men will be maintained.

QUEENSLAND FRIENDLY SOCIETIES.

At a special meeting of the Queensland Friendly Societies' Association held at Brisbane on August 26th, the following resolution was carried unanimously:—

> "That the Government be approached and requested to suspend so much of the Friendly Societies Act as to permit the benefit funds to be used by way of loan in payment of contributions of members unable to make that provision owing to the present crisis."

It was pointed out that an effort would have to be made by the individual society to retain their members who through the unfortunate war might be unable to keep up their subscription,

Abstracts from Current Medical Literature.

OPHTHALMOLOGY.

(100) Treatment of Trachoma.

In dealing with the operative treatment of trachoma, Coover ("Ophthalmology," October, 1912) first describes the procedure of crushing with Knapp's and Noyes' forceps, and Heisrath's incision of the recto-tarsal fold—His own operation is conducted under general anæsthetic. No. 0 or 00 sandpaper, free from glass, is cut into strips three or four inches in length by three-fourths of an inch in width. The upper lid is thoroughly everted, and fixed by Darier forceps, which should lock. A horn spatula protects the cornea. A strip of sandpaper is rolled over the index finger, and holding it firmly between the finger and thumb, the entire lid surface and exposed retro-tarsal folds are thoroughly and briskly rubbed. By holding or rolling the strip of paper in different ways all the recesses can be reached, and the entire surface rubbed down smooth. If necessary the lower lid can be treated in the same way. For a few days after the lids are painted with silver nitrate, 2gr. to the oz., and then 1 per cent. ichthyol in vaseline is used once daily, or 1 in 500 solution of sulphate of copper, until all induration and thickness disappears, which occurs in from four to six weeks. The author concludes that operative treatment yields better results in the majority of cases than medical, that his own operation is applicable to all forms of the disease, and that there is very little reaction.

(101) The Protection of Eyes in Industrial Processes.

Luckiesh (Archives of Ophthalmology, July, 1914) presents a series of spectrograms, the results of elaborate experiment, illustrating the transparency of various media to rays of different wave length. The destructive effect of ultra-violet rays upon animal tissue has long been recognised; another danger is excessively bright images upon the retina, and possibly the absorption of excessive energy by the eye media. This may be the cause of the frequent occurrence of cataract in the tropics. Glass-blowers' cataract cannot be due to ultra-violet rays, as molten glass emits an exceedingly small amount of them. Few of the ultra-violet rays reach the retina as they are absorbed by the cornea and lens. Smoke glasses cannot be recommended for the protection against excessive ultra-violet radiation. The amber glasses absorb the ultra-violet rays, but objection is raised to their use on account of their colour, and they are disliked by the workmen. The most satisfactory glass is the akopos, which is a light yellowish green, gives no objectionable colour, absorbs ultra-violet rays, and is liked by the men. In processes where excessive heat is encountered the goggles must have no metal parts touching the skin. The lenses must be of sufficient mechanical strength, must absorb excessive ultra-violet rays, must be of sufficient density to reduce retinal images to a safe brightness, and must be as free from colour as possible. For arc welding, a very dense neutral tint glass, combined with akopos, is recommended. Great advantage was experienced by actual consultation with the workmen.

(102) Post-operative Complications of Cataract Extractions.

In a discussion which took place at the Ophthalmological Society of the United Kingdom, on April 24th, 1914, Collins (Proc. Ophthalm. Soc., U.K., 1914) first dealt with the subject of grooving of the surface of the wound, which follows wide separation of the front part of the corneal wound. This complication may be met with when the cut out of the knife is more sudden and more at right angles to the corneal plane than it should be. In the next place, he spoke of epithelial down-growths and cysts of the anterior chamber and cornea. These down-growths and cysts arise from the surface epithelium, and extend downwards more deeply than usual. The epithelium may spread over Descemet's membrane, and over the lens capsule and iris. In such cases an increase of tension develops. They are best avoided by an oblique incision and a conjunctival flap.

In regard to down-growth of conjunctival blood vessels between the lips of a wound, he stated that they occurred when a conjunctival flap had been made and there was delayed agglutination of the edges of the wound. They may also follow the entanglement of iris in the incision. He then passed on to the consideration of delayed union of the posterior surface of an extraction incision, haze of cornea in vicinity of incision, and striated opacity of the cornea. These were the result of the interposition of iris, lens capsule, or vitreous between the cut ends of Descemet's membrane. The aqueous filtered into the substance of the cornea, causing swelling and opacity. The temporary striated keratitis was due to the rucking of Descemet's membrane following this corneal swelling. In regard to prolapse of the iris, and cystoid and bulging scars, he said that complications of this nature rendered the eye liable to suppurative inflammation of the uveal tract months or years after the healing of the wound. He was of opinion that astigmatism tended to diminish in amount as cicatrisation proceeded. The curvature of the cornea at right angles to the wound was diminished, and that parallel to it increased. It was due to the overlapping of the scleral by the corneal flap, and was greater the more limited the incision.

Suppurative inflammation starting in the cornea was most commonly due to pneumococci; other organisms found were the staphylococci, pyogenus aureus, ozæna bacilli, the bacilli of the diphtheria group, and streptococci. The percentage of suppuration at Moorfields Hospital had diminished from more than 6 per cent. to 1.15 per cent. during the last 30 years, as a result of greater precautions in eliminating infection from the lachrymal sac, conjunctiva, and margins of the lid. It was also due to sterilization of instruments and dressings, and the use of local anæsthesia. If unchecked suppuration of the cornea would lead to pan-ophthalmitis. Favourable results were sometimes obtained by the use of the galvano-cautery.

Post-operative conjunctivitis might, he said, be a recurrence of a previous conjunctivitis which had been partially cured, or might arise from undue irritation, caused by too strong antiseptics, or from laceration with fixation forceps, or the presence of blood and lens matter in the sac, or from spasmodic entropion of the lower lid.

Turning to the complicated wounds of the iris, non-septic synechiæ, and transient keratitis-punctata, he stated that the posterior synechiæ were due to the iris coming into contact with the torn lens capsule, which resulted in an exudation being thrown out. Atropine was indicated for this condition. The transient keratitis punctata might be due to iris cells, liberated by the iridectomy, clinging to the cornea and causing some reaction there. Under the heading of "Septic Endophthalmitis and Sympathetic Ophthalmitis," he dealt with cases of severe iridocyclitis, which endanger the other eye from sympathetic ophthalmitis. The author had five cases of the latter after 518 extractions. Endophthalmitis was due to infection with organisms which were saprophytes in the normal conjunctiva. Their entrance into the eye was favoured by delayed union, and the presence of lens capsule or vitreous in the wound.

Glaucoma after extraction was generally associated with adhesions of the lens capsule to the extraction scar.

Expulsive hæmorrhage was rare; in only one of the author's patients was the eye lost in this way. The hæmorrhage was from the choroidal vessels.

Detachment of the choroid and of the ciliary body was probably very common. It was temporary and difficult to demonstrate clinically. Opaque membranes were due to retained lens matter in the capsule, to proliferation of capsule cells, or to the formation of adventitious fibrous tissue. The use of capsule forceps tended to lessen their occurrence.

Vitreous opacities and detachment of the retina were more common after loss of vitreous.

(103) Blood-staining of the Cornea.

Begle (Archives of Ophthalmology, July, 1914) deals with the microscopical and chemical analysis of four cases of blood-staining of the cornea, from Elsching's clinic. Case 1: Marie V., 37 years, was struck in the right eye by a piece of wood six weeks before admission. There was hyphæmia largely

covering the iris. The cornea was dull, and presented a large yellow greenish opacity. The eye was exentrated, and microscopical examination of the cornea showed a granular appearance of the corneal lamellæ, which, under a high magnification, was seen to be due to the presence of innumerable small, colourless, highly refractile bodies of round or oval form. Case 2: F. S., aged 18, received a blow on the right eye, which caused a wound of the conjunctiva and sclera, with prolapse of uvea and vitreous. The anterior chamber contained blood, and at the end of about 10 days a reddish grey disc was established in the cornea. The eye was enucleated, and in the cornea were found the highly refractile bodies, in all situations except at the periphery. A few yellowish pigment granules were found in some of the corneal spaces. Case 3: J. B., aged 24, had his left eye injured by a stick when 5 years old. The eye was infected and watery, the cornea dull and stippled, and exhibited a central olive-green disc-shaped opacity 8 mm. in diameter. There was some hyphæmia. The eye was enucleated, and the same small bodies as were found in the previous cases were seen thickly disseminated all over the corneal parenchyma, except at the periphery. Case 4: L. K., aged 19, had been infected with gonorrhœa and syphilis, and in spite of salvarsan and mercury became affected with iridocyclitis three months later. Puncture of the anterior chamber and several iridectomies were performed. About 12 months later he was seen by Elsching, when he showed signs of mental disturbances. The inner and upper part of the cornea was occupied by a deep yellowish green disc-shaped opacity. In the anterior chamber was a large hyphæmia. The eye was enucleated, and refractile bodies were found in the cornea, and a few brown pigment particles in the cornea interspaces. There were no free blood cells in the corneal tissue.

Commenting on these cases, the author states that "spectroscopic finding conclusively establishes the fact, already suggested by other evidence, that in this condition a solution of hæmoglobin is imbibed by the cornea," and causes the greenish or brownish discoloration. The investigation of the nature and origin of the refractile bodies was not conclusive, but suggests that they were some elementary split product of hæmoglobin not containing iron.

(104) Blindness as a Symptom of Hysteria.

Ames' first patient (Archives of Ophthalmology, July, 1914) was a girl of 26 years, who lived a life of drudgery and hard work on a small farm, where she kept house for her father and her younger brothers and sisters for 11 years without a holiday. On being admitted to hospital she had only perception of light; the pupils were round, equal, and slightly sluggish, and the eyeballs were held in a fixed stare. She was quite willing to answer direct questions, and ultimately admitted that blindness was the most comfortable way of escaping her work at home. An electric light was just over chair, and she was told to switch off the light. She recognised the darkness. Then she was told that she herself had the power to turn on her eyesight as she had to switch on the electric light. She did this, and could see well. The next case was a man of 39, who became totally blind overnight. No physical abnormality was found, and Holden diagnosed his condition as hysteria. He was unwilling to talk about himself, and denied personal troubles. He was induced to talk about his dreams, and indirectly confessed that he had trouble with his wife, did not wish to see her, and that he had troubles concerning his employment. He was then made to realise that his blindness was the expression of his desire not to see his wife. He, too, turned on his own eyesight with the electric light.

LARYNGOLOGY AND OTOLOGY.

(105) Suppurative Otitis Media.

J. S. Fraser (Journ. Laryng., June, 1914) reports the case of a boy aged 12, who died of acute suppurative otitis media, purulent labyrinthitis and leptomeningitis. The illness began suddenly with headache, malaise and vomiting. There were some choreic movements of the hands on the day of onset. He had frequent attacks of restlessness, and screamed when touched. He lapsed into a semi-comatose condition on the third day of illness. The pupils were semi-dialated, the conjunctival reflex was present, and there was conjugate deviation of the eyes to the left. There was no distinct retraction of the head; ankle clonus was absent; Kernig's sign was present. There was no discharge from the ears, and no definite tenderness over the mastoid. Cerebro-spinal fluid obtained by lumbar puncture contained pus, with many polymorphs and capsulated diplococci. The temperature was high. Death took place on the fourth day. Post-mortem examination revealed pus in the subarachnoid space on both sides. The cerebro-spinal fluid was turbid at the base of the brain, and there was some œdema. The mastoid cells were filled with pus, as were the left inner ear and auditory meatus; the whole of the right ear, with the exception of the labyrinth, also contained pus. A yellow purulent exudate was present beneath the dura throughout the whole length of the spinal cord. The infection involved the nose, trachea, bronchi, and air passages. Streptococcus mucosus was found in the pus. The membrana tympani were intact on both sides.

(106) Reflex Conditions of the Ear, Nose and Throat.

According to C. E. Ide, the part played in the causation of pathoiogical conditions of the ear, nose and throat by affections of the teeth, jaws, or parotid gland has been greatly neglected (Laryngoscope, Feb., 1914). Ide regards the reflex changes as partly vasomotor and partly nutritional. Nerve irritation results either in a failure of nutrition with local necrosis, due to spasm of arterioles, or in a pseudo-inflammation. After describing in some detail the anatomy of the nerve supply of the organs involved, the author indicates the paths by which the reflex changes take place. Passing to the illustration of his subject, he cites the case of an unerupted impacted wisdom tooth as the cause of reflex pathological conditions. Other causes observed included alveolitis, alveolar abscess, pyorrhœa, necrosis, hyperylastic of dentine and parodontitis. He gives a description of 15 cases coming under this category. The first patient complained of pain in the left eye, with tenderness over the infra-orbital foramen. No local cause for the pain could be detected, but atrophy of the anterior extremity of the inferior turbinate was discovered. Further investigation revealed an alveolar abscess. The eye and the nose symptoms disappeared after the dental trouble had been treated. The second case was one of episcleritis, with boggy left eyelid and photophobia. No definite cause of the condition could be found, but alveolitis was present, and after the offending tooth had been extracted the eye trouble cleared up. The third case was one of pain and swelling of the right external auditory meatus, and of the right lower lid. The cause was shown to lie in an alveolitis. The fourth case was one of pain in the right ear, and yellow discharge from the nose. Here again the only pathological changes present were in the molar teeth. The remainder of the cases were examples of typical affections of the nose, throat or ear, which were caused by affections of the teeth or of the alveoli.

(107) Herpes Zoster of the Auricle.

H. J. Davis reports a case of herpes zoster of the auricle and mastoid regions (Journ. Laryngology, June, 1914). The patient was sent to him as a case of acute mastoid disease. There was some œdema and tenderness behind the left ear, accompanied by a papular and vesicular eruption. The eruption had been preceded by great pain, which ceased as soon as the herpes appeared. The author considers that the cervical plexis was at fault. Urban Pritchard (idem) deals with a case in which all the signs of acute mastoid disease, with complete facial paralysis, were present. The hearing was normal. A few days later three spots appeared on the concha. The herpetic nature of the eruption enabled him to make a diagnosis. He gave a good prognosis, and four months later the facial paralysis had cleared up.

PROPHYLACTIC ANTI-TYPHOID INOCULATIONS FOR NEW SOUTH WALES.

The prophylactic value of inoculations by vaccine against typhoid fever seems now to be securely established by army statistics from the Continent of Europe, from the British Empire, and from America. In the last army manœuvres in the United States, typhoid fever seems to have been eliminated, as far as can be judged, by the application of anti-typhoid inoculation. A recent cablegram from England stated that all troops going on active service were being protected in this manner, and the military and naval authorities in Australia have followed suit, all the men leaving Australian shores receiving prophylactic injections of bacillus typhosus. The friends and relatives of the men concerned will naturally hear of their being so treated, and also that the reactions are as a rule so exceedingly mild as to be barely noticeable. The men, moreover, have been, in many instances, recruited from country districts where typhoid is prevalent. The present time is, therefore, opportune for the consideration of the introduction of preventive measures on a large scale. It is anticipated that within a few months enteric fever will again become prevalent, and it seems advisable to induce as many persons as possible in districts where the disease is endemic, to be protected by prophylactive inoculations. The Director-General of Public Health has authorized the announcement that anti-typhoid vaccines can be obtained by any medical practitioner free of cost, for the purpose of offering the residents of his district this opportunity of being protected. The applicant should state the number of persons he proposes to treat, and is requested to enter the details of the inoculation in a book, or on a paper, a duplicate of which should be forwarded to the Department of Public Health for statistical purposes.

It may be well here to call attention to some of the aspects of anti-typhoid inoculation. First of all as to the wisdom of the method from a purely economic aspect. During the last ten years 22,756 cases of typhoid fever have been officially notified in New South Wales. This gives an average yearly incidence of 2275 cases. At a low estimate it may be considered that each individual patient is prevented from carrying on his work for a period of at least five weeks. Each patient will have been prevented, by his illness, from contributing to the community work of a value of about £2 per week. In children of course the value of their work is practically nil, and in women in many cases it does not reach as high as £2. But on the other hand, the value of the work of many men probably far exceeds the estimate. Each case, therefore, of typhoid fever, may be considered as preventing a supply to the common weal of £10 worth of work. If to this be added a further £1 per week representing the extra cost involved by the illness for hospital attention, medical service, drugs, etc., the average loss to the State for each case may be estimated at £15; in other words, the prevention of a single case of typhoid fever will mean that the community as a whole is richer thereby to the extent of about £15. It would, therefore, be richer to the extent of £34,125 yearly if typhoid fever were eliminated from the State altogether, and none of the average 2275 cases occurred. There is a further additional loss to the State due to the elimination of workers by death. During the last ten years 1984 deaths have occurred in New South Wales from typhoid fever. If on an average each individual be considered to contribute £100 worth of work a year to the community, and also if in any individual case the expectation of life at the time when death occurred be taken as ten years, the State would be losing during any particular year £100 for each death occurring in that year, together with £100 for each death which had occurred in the nine preceding years. During the present year, 1914, therefore, it may be considered that New South Wales is losing £198,400 through deaths from typhoid fever during the preceding ten years; the financial position of the State would be increased by this amount had these deaths been prevented.

It is therefore reasonable to assume that at a rough estimate New South Wales is losing during the year 1914 very nearly a quarter of a million pounds (£232,525) on account of the existence of typhoid fever. It may further be assumed that for every case of typhoid feved prevented the State would gain at least £15, and for every seven cases—since the mortality rate is probably about 7 per cent., an additional sum representing £100 a year for the ten following years. If, therefore, the occurrence of enteric fever could be prevented to any extent at a small cost as regards money, time and convenience, by means of anti-typhoid inoculation, the wisdom of taking advantage of these preventive measures must be patent to all. The Department of Public Health has recently supplied the Commonwealth authorities with sufficient anti-typhoid vaccine to protect about 22,000 men. The actual cost of preparation of this large amount of vaccine is probably less than one penny per case. It will, therefore, be seen that as regards cost to the health authority of any particular State, the wholesale preparation of a vaccine of this nature would be a trivial sum. For £1000 probably vaccine could be supplied more than sufficient to protect a quarter of a millon people. It is unlikely that such a number is exposed to infection.

The anti-typhoid vaccine now issued by the New South Wales Board of Health is being prepared from nine different tested strains of bacillus typhosus, isolated from various cases of typhoid fever; it is therefore a polyvalent vaccine. Some of the strains have been isolated quite recently, while others were isolated during the past ten years and more. The strength to which it is made up is 1,000,000,000 bacilli per c.cm., suspended in physiological saline solution to which 0.5 per cent. of carbolic acid has been added. The bacilli are killed by exposing them to a temperature of 55° C. for an hour prior to emulsion. The doses recommended are an initial one of 250,000,000, and a second dose a week later of 500,000,000. It is advised that a third dose of from 500,000,000 to 1,000,000,000 be given a week later. The injection is best made into the loose tissues of the upper arm alternately on each side. The injection is followed in a few hours by an area of redness and slight swelling, accompanied by tenderness. At the end of 24 hours there may be a very slight rise of temperature up to 99°, and there may be very slight and indefinite malaise. In some cases practically no disturbance at all is caused, while in a few individuals, apparently slightly more sensitive, there may be a certain amount of sleeplessness, and the patient may desire to go to bed.

As regards the persons whom it is advisable to have protected in this way, it may be pointed out that all medical men, nurses, wardsmen and laundry maids in connection with hospitals throughout the State should be protected. It is unnecessary for the general population of a large, well-sewered city like Sydney to be protected, because the incidence of the disease amongst the inhabitants is very small. In certain country towns, however, there are a certain number of cases of typhoid fever in each year. Not only the inhabitants of these places, but also persons visiting them will find it advisable to be protected by inoculation. In Broken Hill, for instance, during the last few years, there have been more than one hundred cases of typhoid fever annually. Regular cases, though in smaller numbers, appear at Cobar, Forbes, Tamworth, and several other country towns. It is highly desirable that persons dwelling in, or visiting these places be protected, as well as all those dwellers in towns and villages where there is an annual appearance of a few cases of this disease.

It is to be hoped that practitioners will assist the New South Wales Board of Health, as far as lies within their power, in subjecting a very large number of persons exposed to infection to this form of prophylactic inoculation. Private practitioners are requested to carry this out at as reasonable a cost to individuals as is possible. It is essential in the interests of the community to prevent the occurrence of typhoid fever in country towns, and the protection of the individuals of these towns through their own practising medical men is preferable to the possible intervention of the State.

ZOOLOGY SECTION, BRITISH ASSOCIATION.

(Continued from page 231.)

sary for Prof. Bateson and his school to go out into the field and study plants not under artificial, but in their natural surroundings.

Mimicry.

In dealing with the subject of mimicry in Australian insects, Professor E. B. Poulton, F.R.S., said that it was extremely interesting to compare the phenomena of mimicry as they are exhibited in the different parts of the world. It was found that the models in each tropical region are, as a rule, related, and often very closely related, to those of the other regions. Nevertheless, in spite of these relationships, the models commonly exhibited patterns which were peculiar to each region. Thus the Danainæ and their allies, the tropical American Ithomiinæ, always tended to be mimicked by other butterflies, although their patterns in each of the great tropical regions were for the most part very different from those in the others. The same conclusions emerge when other great groups of models were compared, and the whole body of facts afforded strong indirect evidence in support of the hypothesis that mimicry was an advantageous resemblance which had grown up under the influence of natural selection.

Australia was the most isolated of all the inhabited continental tracts on the earth's surface, and its isolation was reflected in its peculiar fauna and flora. How far was it reflected in the insect-models and their mimics? Up to the present time, the subject had been but little studied in Australian material, but, nevertheless, certain conclusions of much interest could be arrived at.

Perhaps the most widely spread models in the world were the black yellow-banded stinging hymenoptera. The central members of these powerful combinations were wasps (diploptera), around which were ranged sand-wasps (fossores) and, in far smaller numbers, bees (anthophila), followed by mimetic species of the phytophagous hymenoptera, and of other orders—diptera, coleoptera, lepidoptera, etc. Throughout this dominant combination of models and mimics the subcylindrical body was black, encircled by many bright yellow bands. Although widespread over the world, it was especially powerful in the north temperate zone. In Australia, however its place was taken by a combination with a very distinct pattern. The bands were deep brownish orange instead of bright yellow, and they were few and broad, instead of many and narrow. This pattern ran through a large and complex set of models and mimics. It was very convincing to compare such a mimetic asilid fly as the European asilus crabroniformis with the Australian species, and to observe how their very different patterns resemble those of the respective aculeate models. An equally significant comparison might be drawn between the mimetic longicorn beetles of these two parts of the world.

The conspicuous sluggish lycid beetles formed another dominant group of models in all the tropical regions, and here, too, a powerful Australian combination exhibited a peculiar colouring, and in some respects a peculiar constitution.

Material already received from Commander J. J. Walker in the Sydney district and from Mr. A. Eland Shaw at Healesville, Victoria, showed that the Australian contribution to the study of mimicry was sure to be of the highest interest and importance.

SYDNEY MEETING.

(By our Special Correspondent.)

August 21st, 1914.

Trypanosomes in the Invertebrate Host.

In his communication on the development of trypanosomes, Professor E. A. Minchin, F.R.S. (London), said that if an analysis and comparison be made of those instances in which it can be claimed that the development of a given species of trypanosome in its invertebrate host is known in at least its principal traits, it was seen at once that in every such instance there was a part of the developmental cycle which is constant in occurrence and uniform in character, and another part which is of inconstant occurrence and very variable in character.

In the constant part of the cycle the parasite always assumed the crithidial type of structure and multiplied incessantly in this form to produce a lasting stock of the parasite, certain individuals of which change sporadically from the crithidial into the trypaniform type, and so became the final, propagative form of the development, destined to pass back into the vertebrate host and establish the infection in it. During hunger-periods the crithidial forms might pass temporarily, in some cases, into the resting, non-flagellated leishmanial form, until food was again abundant, when they formed a new flagellum and reverted to the crithidial type of structure.

The inconstant part of the cycle, when it occurred, was intercalated at the very beginning of the development in the invertebrate, and lasted but a relatively short time; it was derived directly from the trypanosomes taken up by the invertebrate from the vertebrate host, and took the form of an active multiplication of the parasites in either the trypaniform or leishmanial condition. In the cases where this early multiplicative phase was wanting altogether, the trypanosomes taken up by the invertebrate host passed at once into the crithidial phase.

When a further comparison was made between the development of trypanosomes in the invertebrate host and the development of the closely allied species of Crithidia and Leptomonas which had no alternation of hosts or generations, but were confined during their entire life-history to particular species of invertebrate hosts, it was seen at once that the life-cycles of these parasites of invertebrates were similar in all essential points to the crithidial phases of trypanosomes in their invertebrate hosts. It was evident, therefore, that the crithidial phase in the development of a trypanosome was to be interpreted as a reversion to, or recapitulation of, the type of development that occurred in the ancestral form which was originally a parasite of the invertebrate alone, before it had obtained a footing in the vertebrate host or had acquired the trypanosome-like type of structure; while the multiplicative phases of variable character preceding the crithidial phase in trypanosome-development was to be regarded as having been intercalated secondarily into the life-cycle and of no phylogenetic significance.

Red Blood Corpuscles.

Dr. J. Burton Cleland (Sydney) spoke of the relative size of the red cells of some vertebrates.

In searching blood-films from Australian birds for parasites, it was noticed that the red corpuscles of a heron were distinctly larger than those of the various Passerine birds examined. This led to systematic measurements of the sizes of the red cells of various Australian vertebrates. The slides examined have been all stained by "dry" methods, wet fixation and staining methods being impracticable in the field. Experience showed, however, that this method might be relied on for the purpose in view.

Amongst the fishes, the Dipnoi had enormous red cells, those of Ceratodus forsteri being 39 by 23 to 25 μ. The Elasmobranchs had also large cells, varying from 18 by 126 to 23 by 13.5 μ. Amongst Teleostean fishes the size was much smaller. The cells were also rounder. In Therapion unicolor they were nearly spherical in size 6 to 8 μ. Other species range from 9 by 7 up to 13.5 by 10.3 μ in a catfish.

The reptilia, snakes, lizards, and tortoises had red corpuscles ranging usually from 16 to 21 by 9 to 11 μ, though in some cases, as in the genus lygosoma, the size tended to be smaller (14 to 16 by 8 to 10 μ).

Batrachians showed red cells usually of from 18 to 20 by 10 to 14 μ.

Amongst birds, the emu had the largest (15.5 to 16.5 by 8.5 to 9.5 μ. The Podicipediformes, Spheniscifformes, Ardeiformes and Pelecaniformes came next (approximately 14 by 8 μ). Charadriiformes were generally a little smaller. The pigeons, hawks, parrots, kingfishers, and cuckoos came next, the kingfishers being perhaps the largest of these. In the Passerine birds there was a definite tendency to smaller cells, ranging from 10 to 12 by 5 to 7 μ, with the exception of the family Corvidæ, where the size approximated more to the previous group.

These figures seemed to indicate that with specialisation had eventually come, both in fishes and in birds, a diminution in size of the red cells. The cumbersome corpuscles of Ceratodus had doubtless played a part in the gradual extinction of the Dipnoan fishes. The relationship of the various classes to each other was clearly shown in the size of the red cells.

Australian Hæmatozoa.

Dr. J. Burton Cleland (Sydney) said that owing to the geographical isolation of Australia, the study of the blood parasites of the vertebrates, especially of such as have no easy means of passing over stretches of ocean, was of considerable interest. In some cases, such as the marsupials, interesting speculation arose as to whether the Hæmatozoa found in them reached Australia (1) with the marsupials when these originally came; or (2) as parasites of the invertebrate host by a separate arrival; or (3) whether their appearance represented the adaptation in Australia of a parasite, at one time confined to an invertebrate host, to a habitat partly in the vertebrate and partly in the invertebrate host.

In marsupials Hæmogregarines had been found. Breinl had recorded in a bat the presence of a Trypanosome and of a Plasmodium. In birds, Plasmodium precox has been recorded in a falcon, and a Plasmodium had been found in the black swan. Plasmodium had also been recorded in the introduced sparrow. Plasmodium seemed to be rare in birds compared with the presence of Halteridium. Halteridia are common in Australian birds, and had been found in all the States with the exception of Tasmania, though they had been found on Flinders Island in Bass Straits. The appearance of the forms found varied somewhat, suggesting specific differences. Trypanosomes had been found in several species, but seemed confined more especially to Queensland and northern New South Wales. With the same distribution, and often in the same infected birds, large parasites might be found in distended red cells. The parasite in the red cell was spherical and indented the nucleus of the host-cell, which was stretched over it so as to form a cap. The speaker was of opinion that this was the intracorpuscular form of the Trypanosome with which it was usually associated, although Dr. Harvey Johnston, who was associated with him in first describing this form, has since referred to it as a Leucocytozoon, as did Breinl. The corpuscles were certainly not elongated in the remarkable way in which they were in infections by Leucocytozoon ziemannii. Microfilariæ were common in birds.

Reptiles.—Hæmogregarines were common in snakes and lizards. Trypanosomes and Hæmogregarines were met with in tortoises, as well as a Hæmocystidium. A Hæmocystidium had been met with in a gecko, and microfilariæ in the water lizard, Physignathus lusueurii.

Amphibia.—A Hæmogregarine had been met with in one species of frog only. Trypanosomes in several species.

Fishes.—Trypanosomes had been found in the freshwater eel and in a catfish.

Professor Minchin made a few remarks on the subject.

Public Health.

THE HEALTH OF BROKEN HILL.

In February last the Medical Officer of Health for the Broken Hill district presented his annual report for the year 1913 to the City Council. The chief points in his report were as follows:—With a population of 35,000, a satisfactory birthrate of 39.82 per 1000 had been registered. The death-rate was 18.6 per 1000 as compared with 13.08, the average rate for the three preceding years. The mortality of children under 12 months worked out at 148.38 per 1000 births. This was compared with the rate for the whole State, which was 70. There were 210 cases of typhoid, of which 19 were fatal. There were 641 cases of diphtheria, of which 33 were fatal, and 64 deaths from pneumonia were registered during the year. The Medical Officer of Health attributed the high death-rate, to a certain extent, to the high birth-rate, to the severe climatic conditions existing during the year, and to the unsatisfactory condition of the milk supply; the chief factor, however, was, in his opinion, the poor condition of the sanitation of the town. He pointed out that it was essential that the orders given by the Health Inspectors should be carried out. By means of paving and grading of yards and lanes, and slight alterations in the structure of water closets and privies, considerable improvement would be effected. The slop pits in many of the back yards were most evil-smelling; they should be provided with properly fitting covers, and high ventilating shafts. The most severe indictment, however, which he dealt to the Council was in regard to the slop carts and pans. He pointed out that the carts were of very bad construction, and that they emitted most foul smells in their passage through the streets. The pans were too few in number, and provided with lids which did not seal them effectively. He set his face against the proposal that the collection of garbage and slop-wash, etc., should be carried out at night-time, on the ground that this was no remedy. He designated the old night-cart service, which is still used in parts of the town, as disgusting. In conclusion he stated that if the municipality offended against the law in a flagrant manner they could not expect the citizens to obey the orders of the municipal sanitary officers, and that he proposed to request the Central Board of Health to enforce compliance with Ordinance 45, Section 19a in all particulars.

In his quarterly report, submitted to the City Council on April 23rd, the Medical Officer of Health had to record a still worse state of affairs. The death-rate showed an increase equivalent to 1.41 per 1000 per annum, as compared with the corresponding quarter of the previous year. The infantile mortality and the incidence of infectious diseases had also increased. In speaking of the insanitary condition of the town, he stated: " . . . and is it not strange that the municipality calmly acquiesces in this condition, and cannot find time to consider works that I have pointed out as urgent; and also that they flagrantly break the law, and not only allow pollution but are the worst polluters themselves." The result of this report was that instead of submitting it to the Health Committee for consideration, as had been done in the past, with the result that nothing more was heard of it, the Council passed the following resolution:—

> "That the Health Committee be requested to report specially to the Council on the matters referred to by the officer, and especially in respect to the accusasations made."

The Medical Officer of Health presented his report for the quarter ended June 30th to the Council of Broken Hill on July 23rd: 157 deaths had been recorded during the quarter, which was equivalent to an annual death-rate of 18.23; 181 cases of typhoid fever were notified as compared with 81 for the corresponding period in the previous year; 183 cases of diphtheria were notified as compared with 211 for the second quarter of 1913; and 21 cases of scarlatina as compared with 7. There had been 24 deaths from typhoid fever, and 8 from diphtheria. In commenting on the figures, especially those dealing with enteric fever, the Medical Officer of Health stated that municipal neglect was largely responsible for the unsatisfactory state of affairs. The Chief Sanitary Inspector reported that he had caused a house-to-house inspection to be made of the congested part of the town, and that the notices which had been served, requiring dirty premises to be cleaned up, had been complied with.

Dr. J. F. Bartley, Government Medical Officer, and Medical Officer of Health of the Broken Hill district, gave evidence before the Royal Commission appointed to inquire into the working and living conditions of miners at Broken Hill, on August 2nd, 1914. In dealing with the high incidence of pneumonia among underground workers, he stated that there were other factors contributing to this state of affairs, besides the industrial causes of the disease. These other causes, as well as those which had led to the unusual prevalence of enteric fever at Broken Hill, were to be sought in the defective sanitary condition of the town.

SMALL-POX IN SYDNEY.

The number of small-pox cases, reported to the Department of Public Health, New South Wales, during the week ended 23rd August, 1914, were:—

City of Sydney and Metropolitan District	Cases.
Country—Helensburgh	17
Quirindi	1
	2
Total	20

The number of small-pox cases reported to the Department of Public Health, New South Wales, during the week ended 30th August, 1914, were:—

	Cases.
Metropolitan District	4
Country—Moree	4
Quirindi	2
Total	10

Obituary.

SIR NORMAND MacLAURIN.

By the death of Sir Normand Maclaurin the medical profession, in common with the rest of the educated community, has lost one whose works and example have contributed toward the improvement of the conditions of mankind. In his capacity as a medical practitioner he exercised those qualities which make for the mental and bodily recovery of patients. He was not satisfied to deal with pathological conditions alone; he always had consideration for the psycological conditions of those who came under his care, with the result that the effect of contact with him was increased happiness. In the same way, his activities in University life and in politics increased the sum of human happiness. He was possessed with qualities which rendered all the work which he undertook of peculiar value. He combined tact with knowledge, dexterous skill with courage, and caution with kindness.

We are in the particularly fortunate position of being able to publish an account of Sir Normand MacLaurin's achievements, and an appreciation of his qualities, written by his son, Dr. Charles MacLaurin. With him, we mourn the loss of a great man, and of a friend. He will not be forgotten.

Dr. Charles MacLaurin writes:—

I have been asked to write an account of my late father for the medical profession of Australia. With great reluctance I do so, for it is in general unseemly that a son should write of his parents; yet inasmuch as I write for my professional brethren (who in this regard have indeed proved as a band of brothers), it seems fitting that the ordinary canons of propriety should be momentarily set aside. If I do unwittingly yield to threnodism be it remembered that I am writing of one whom his intimate friends held as among the greatest men of our time; if I do indecorously rend the veil which custom has ordained should conceal the most sacred feelings, let it be remembered that I am writing of one who to me—and no man knew him as I did—has from my earliest childhood stood on the highest pinnacle of human goodness.

The facts of his life are these. He was born at Kilconquhar, in Fife, December 10th, 1835; his parents were James MacLaurin, an able schoolmaster, and Catherine Briarcliffe, a lady of unusual courage. He took every prize open to him at St. Andrews, where he became Master of Arts; he became M.D. of Edinburgh in 1857. He went to Spain and to Mogador in 1858, with his dying brother, who, had he lived, gave promise of being the greatest preacher in the Church of Scotland. He entered the Navy in 1859, and served in the Royal Albert, Marlborough, and Greenwich Hospitals; and finally came to Sydney in the Challenger. He settled at Parramatta in 1871, marrying Eliza, daughter of Charles Nathan, F.R.C.S.; he moved into Macquarie Street in 1872, and practised his profession till he was about 70 years of age. He entered the Senate, and became successively Vice-Chancellor and Chancellor, the latter in 1896. He was Health Officer for Sydney; was appointed to the Legislative Council; joined the Government at the time of the bank crisis; later was offered and refused the Premiership; was knighted in 1902; and died on August 24th, 1914, holding, so far as I remember, the following positions: Chancellor of the University, member of the Legislative Council, member of the Board of Health, trustee of the Public Library and the Grammar School, Director of Prince Alfred Hospital, the Bank of New South Wales, the Sugar Company, the Commercial Union Assurance Company, and Chairman of the Mutual Life and Citizens' Assurance Company.

He landed here at 36, an unknown doctor, with neither friends, money, nor influence; 23 years later he was called to the leadership of the nation at the time of its greatest danger; 20 years later still he was laid to rest amidst a scene of public grief which is almost unparalleled in our annals. For among the thousands of letters which we have received are many which breathe a sorrow so intense that one would almost feel that these friends had lost a father.

He was humble, and free from arrogance. He persistently refused his knighthood; when at last it was conferred upon him he knew nothing about it until I rang him up early in the morning and told him. It was the only time I ever saw him furiously, almost uncontrollably angry. For a long time he refused to use the title, and we were sternly forbidden to refer to it in his hearing. The old brass-plate of Dr. MacLaurin still hangs on the door of our house in Macquarie St., and will, I hope, hang there till the present generation is gathered to its father. Ultimately he became reconciled to his title, and of late years I think he took a little pleasure in the sonorous name.

In whatever company he was found he was automatically forced into the leadership. Neither old nor young could long resist his charm. Bryce, the great ambassador, Jersey, the great financier, Hogkin, the great historian, William Rutherford, the great physiologist, Henry Jones, the great philosopher, all men of strong, overwhelming personality, all fell under the sway of his magic; and I can see him now, the great Chancellor, stooping with outstretched hands and kind eyes, and a "Well, little Missy," that disarmed even the impenetrable shyness of six years old.

Many men may have rivalled—none excelled—him in courage and administrative capacity; some in learning, a few in infinite sympathy, one or two even in wisdom, but I can recall no man who combined all these qualities in so astounding a degree. To his sons his friendship was so all-sufficing that they formed few other friendships—let those in similar case appreciate the meaning of his going.

He betrayed no friend, and harassed no enemy; he did nothing common nor mean, and he "sympathised with suffering;" his life was utterly pure and faithful, and no unworthy word ever in my knowledge of 40 years, passed his lips. When I reflect on those men whom history has surnamed "The Great," I think it a bitter irony that so noble a word should be used for thieves and murderers, and I compare the little good wrought by these Butcher-Kings with that done for his country by the kind and simple doctor who has just left us.

He had the art of phrase in a superlative degree. He could sum up in a sentence a man or a situation with words either original or drawn from his amazing memory. When he was, as we thought, recovered from his operation, I used to sit and talk with him about the war, in which he was most keenly interested. The evening before his release he suddenly flashed out: "Austria with her idiot Arch-dukes—that was Bismarck's phrase, wasn't it?" That was the last flash of the old wit; six hours later darkness clouded his brain, and the few remaining glimmers of light cannot be told.

It is fitting to leave him, loyal comrade and stately citizen, with the words he loved, the sad words written on many a Roman tomb, "Sit Tibi Terra Levis."

The funeral of Sir Normand MacLaurin was held on August 26th. A very large number of persons attended. The service was conducted at the graveside by the Rev.

Dr. Hay, who was an intimate personal friend of Sir Normand's. The Federal Government was represented by Mr. W. H. Kelly, M.P., the Governor of New South Wales by General Finn, the State Government by Mr. J. Cann, Mr. D. R. Hall, and Mr. F. Flowers, and the Chief Justice of the High Court (Sir Samuel Griffith) by Mr. N. M'Ghie. Representing the University and the Senate were the Vice-Chancellor (Judge Backhouse), Sir William Cullen, Sir T. P. Anderson Stuart, Sir Phillip Sydney Jones, Mr. F. Leverrier, Professors David, Peden, Haswell, Douglas Stewart, Wilson, Warren, Holme, Todd, Watt, Fawcett, Carslaw, Wood, Robinson, and Anderson, Mr. H. C. L. Anderson, Dr. Flashman, Dr. T. Storie Dixson, Mr. H. E. Barff, Mr. R. A. Dallen, Mr. P. Board (Director of Education), Mr. Clive Teece, Mr. R. Teece, and Dr. G. H. Abbott, Mr. Meredith Atkinson, Mr. J. Le Gay Brereton, Mr. W. A. Selle, Mr. W. S. Mayer, Mr. Justice Simpson, Mr. J. M. Purves, Colonel Foster, Mr. J. H. Maiden, and Mr. H. R. Curlewis.

Those representing the Legislative Council were: Sir Francis Suttor (President), Sir Charles Mackellar, Sir Joseph Carruthers. Sir Allen Taylor, Colonel Burns, Colonel Holborrow, Messrs. E. Fosbery, P. E. Winchcombe, W. J. Trickett, J. Garland, J. A. Browne, Earp, Robson, Ashton, Bryant, Farleigh, Holden, Hughes, Kater, Kethel, Meeks, and Travers, and Drs. Doyle, Creed, and Nash, Ms.L.C., and Messrs. W. L. S. Cooper, C. H. H. Calvert, and W. K. Charlton (officers of the Legislative Council), and the Town Clerk.

The British Medical Association was represented by Dr. David Thomas, President of the New South Wales Branch, and Dr. R. H. Todd, Hon. Secretary.

Mr. T. H. Nesbitt represented the Lord Mayor of Sydney.

The Sydney University Undergraduates' Association was represented by H. V. Evatt and W. R. Aspinall (hon. secretary).

The Highland Society, the University Club, the University Medical Society, the University Women's Union, the Mutual Life and Citizens' Assurance Company, Ltd., the University Union, and the Public Library were also represented.

Proceedings of Australasian Medical Board.

SOUTH AUSTRALIA.

The following persons have been registered under the provisions of the Medical Act of 1867 as duly qualified medical practitioners:—

Jeffrey Wilmott Wilkinson, M.B., B.S., Melb., 1914.

On the 13th August—

Roydon Herbert Minton Connell, M.B., B.S., Melb., 1913.

James Charles Robert Lind, L.R.C.P., Lond.; M.R.C.S., Eng., 1911.

Medical Appointments Vacant

DEPARTMENT OF PUBLIC HEALTH, QUEENSLAND.
ASSISTANT HEALTH OFFICER.

Applications from duly qualified medical men not over 29 years of age will be received by the undersigned up to September 27th, 1914. Duties will be at the direction of the Commissioner of Public Health. Salary, £300 per annum.

WILLIAM GALL, Under-Secretary.

Home Secretary's Department, Brisbane.

Diary for the Month.

Sept. 7.—Alfred Hospital (Vic.), Clinical Meeting.
Sept. 9.—Melbourne Pedriatric Society.
Sept. 10.—Victorian Branch, B.M.A., Council Meeting.
Sept. 11.—New South Wales Branch B.M.A., Clinical Evening.
Sept. 11.—New South Wales Branch, B.M.A., Last Day for Nominations for Election to Federal Committee of B.M.A. in Australia.
Sept. 15.—New South Wales Branch B.M.A., Council Meeting.
Sept. 16.—Western Australian Branch, B.M.A., Branch Meeting.
Sept. 16.—Victorian Branch, B.M.A., Clinical Meeting.
Sept. 22.—Victorian Branch, B.M.A., Eye and Ear Section.
Sept. 25.—New South Wales Branch B.M.A., Election of Members of Federal Committee of B.M.A. in Australia.
Sept. 25.—New South Wales Branch B.M.A., Ordinary Meeting.
Sept. 25.—Melbourne Hospital, Clinical Meeting.
Sept. 29.—New South Wales Branch B.M.A., Committee Meeting.
Sept. 29.—Victorian Branch, B.M.A., Eye and Ear Section.
Sept. 30.—Victorian Branch, B.M.A., Council Meeting.
Oct. 1.—New South Wales Branch, B.M.A, Annual Dinner.
Oct. 1 and 2—New South Branch, B.M.A., Annual Meeting of the Delegates of the Affiliated Local Associations of Members with the Council.
Oct. 6.—New South Wales Branch, B.M.A., Council Meeting.
Oct. 9.—New South Wales Branch, B.M.A., Clinical Evening.
Oct. 20.—New South Wales Branch, B.M.A., Council Meeting.
Oct. 21.—Western Australian Branch, B.M.A., Branch Meeting.
Oct. 27.—New South Wales Branch, B.M.A., Committee Meetings.
Oct. 27.—Victorian Branch, B.M.A., Eye and Ear Section.
Oct. 30.—New South Wales Branch, B.M.A., Ordinary Meeting.

Warning Notices.

Medical Practioners are requested not to apply for any appointment referred to below without having first communicated with the Honorary Secretary for the Branch of this Association:—

Appointment.	Hon. Secty. of Branch.
Brisbane United Friendly Societies' Institute, Lodges, etc., of the Longreach, Central Queensland, and Warwick Friendly Societies, Darling Downs, Queensland.	Queensland Branch, B.M.A. Building, Adelaide Street, Brisbane.
Swan District Medical Officer.	Western Australian Branch B.M.A., 230 St. George's Terrace, Perth.
Contract Practice in Western Australia.	Western Australian Branch B.M.A., 230 St. George's Terrace, Perth.
Goulburn Friendly Societies' Association, at Goulburn, N.S.W. Lodges at Casino, N. S. Wales. The United Friendly Soceities' Association of Orange, N.S.W. Friendly Societies' Lodges, Braidwood, New South Wales. The Friendly Societies' Association, Lithgow, N.S.W.	N. S. Wales Branch, B.M.A., 30-34 Elizabeth Street, Sydney.
The Friendly Societies' Medical Association Incorporated, Adelaide.	S.A. Branch, B.M.A., 3 North Terrace, Adelaide, S.A.

EDITORIAL NOTICES.

Manuscripts forwarded to the office of this Journal cannot under any circumstances be returned.

Original articles forwarded for publication are understood to be offered to the "Medical Journal of Australia" alone, unless the contrary be stated.

All communications should be addressed to "The Editor," "Medical Journal of Australia," B.M.A. Building, 30-34 Elizabeth Street, Sydney, New South Wales.

The following periodicals are required by the Librarian of the New South Wales Branch of the British Medical Association to complete the series for binding. Members who have borrowed these journals are requested to return them as soon as possible:—

"Lancet," January 31, 1914.
"Lancet," February 7, 1914.